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Stated Meeting, January 4th, 1894.

OBSERVATIONS UPON ABDOMINAL SURGERY, IN RELATION TO THE GENERAL PRACTITIONER.

By A. VANDERVEER, M. D.,
ALBANY, NEW YORK.

Mr. President and Members:

But little more than three decades have passed since were spoken the words of that particular lecturer whom we all respected, and I may say loved, for his pure teaching, when by a vote of the faculty he was instructed to deliver a course of lectures upon the subject of ovariotomy; he said very quaintly in his opening sentence, "Gentlemen, I shall expect to speak to-day with more than my usual volubility and eloquence, as I shall not be hampered with any knowledge of my subject."

This really reflected at that time the true condition of abdominal surgery in this city. Gastrotomy, gastrostomy, abdominal section, laparotomy, cœliotomy, being terms little used in those days, some even unknown by good specialists, and by many of those who constituted the great family of general practitioners understood not at all.

Note now the change. Of the living members of his audience at that time, as of those who are present here this evening (and the same can be said of any gathering of medical men), there is not one but has some knowledge of the advances made within so brief a period, relating to the subject of abdominal surgery, and in many quiet towns or larger villages and lesser cities are to be found brilliant

and competent operators. No longer do we have trustees of prominent hospitals and medical institutions passing resolutions forbidding abdominal section, but on the contrary we have them leading in their endeavors to do all that is possible in the way of erecting properly constructed buildings—aseptic, hygienic, pure, sweet, clean amphitheaters—in which to operate, providing everything that aids the surgeon who in this field tries to relieve human suffering, or save the lives of those who but a few years since must necessarily have passed away. Operations now performed so successfully were at that time unknown or done amid surroundings and with technique so imperfect that the mortality list shocked rather than inspired confidence in our general practitioners. Now that such advances have been made, what is the true relation existing to-day between the family physician and the surgeon who in his own city or location endeavors to do that which the general practitioner recognizes is necessary for his patient in the way of operative interference, but is himself totally unprepared to do, not only from the personal surroundings of the case, but because of his inexperience and want of knowledge of the surgical technique required?

It is not my intention to inflict upon you this evening a review of the advances made in the domain of abdominal surgery since this memorable sentence was given to the students of the medical department of Columbia College.

There comes before the general practitioner a class of abdominal work in which he has ample time to make the diagnosis, if he be a man of fairly average skill. As a busy practitioner, he can pause sufficiently to examine his case with proper care, and can relegate this class of patients to such hospitals, and into the care of such men who, from experience and careful study, have fitted themselves to claim his confidence and that of his patients.

Slow-growing tumors of the abdomen, of whatever nature, come under this classification, and the general

practitioner of to-day who is conscientious in the discharge of his duties, knows full well that he is no longer to encourage his patient with an ovarian tumor by tapping her from month to month, or week to week; neither is he to attempt that which his own good judgment tells him he is not competent to do in the case of solid tumors, whatever their character, by the not harmless but indiscreet application of electricity.

Such a class of patients he has time to dispose of in a manner acquitting him as an honest practitioner, by placing them upon a plane of surroundings very hopeful for their recovery. But, on the other hand, what are the assurances that can be given to the general practitioner regarding more acute cases? Cases of a character that perhaps he may see but once in his professional lifetime, possibly oftener, yet of which from his text-books he may have a theoretical knowledge, never so valuable as that of practical experience and personal observation. The general practitioner, his patients, the friends of the latter, to-day, in the acute cases that present with abdominal troubles, are keenly alive to the fact that wonderful advances have been made in this branch of surgery within a few years, and look to such men as have, by their success, won and can claim their confidence for help in this direction. Have they been led too far by our text-books of to-day in dwelling upon the work of the abdominal surgeon? Is the material that we read in our medical journals too hopeful, too enthusiastic in presenting this subject to the profession at large? I think not, and yet I am confident that there are certain conditions in which the abdominal surgeon and the general practitioner should be brought into closer contact, into nearer touch, one with the other, and that it is the duty of the experienced operator and the diagnostician in abdominal surgery to place before the profession at large, honestly and clearly, his conclusions as to the diagnosis of certain cases, as to the results

that may be reasonably expected from operations done at certain periods and at certain times in the course of the case. Take for instance the surgery of the gall-bladder of to-day. There are many noble, bright, general practitioners who, upon this subject, have not yet settled the question in their own minds, and it is not altogether clear to them as to the actual good results that have followed the operations upon the gall-bladder and ducts; and they are equally at a loss in making their diagnosis in certain cases, for the reason that they have been taught in the past that one of the most prominent symptoms of biliary colic, and of biliary concretions, is the presence of jaundice, when many of us know full well that in not a few of our operations of this kind this one symptom, upon which so much stress is laid, and has been laid in the past, is frequently entirely absent.

Let me illustrate a little more fully by one good, general practitioner, who has been exceedingly careful in investigating a particular case. No evidence of gall-stone detritus is found in the stools, although for several days they have been passed carefully through a sieve, and every other precaution exercised. No jaundice, no gall-stones in stools, when every form of medical treatment has been followed out! No distention of gall-bladder! Surely the case must be one of sympathetic gastric colic. But the attacks grow more severe and frequent, and the general practitioner decides to send for the surgeon who, he knows, is doing abdominal surgery. A careful examination by the latter, showing the localized tenderness over the region of the gall-bladder, the sudden onset of the attacks, the speedy relief afforded at times when certain positions are assumed, the bringing out of the fact, on careful questioning of both husband and wife, that at certain times they had noticed a swelling where now the point of tenderness seemed the greatest, all these conditions being emphasized to the family physician, resulted in a joint expression to the

patient and her friends that an exploration was justifiable and proper. The opinion was ventured by the surgeon that there was probably a large stone present in the gall-bladder (a condition doubted by the general practitioner, as large stones, the books say, do not produce trouble of this kind), and that the common duct was free; that the gall-bladder was nearing the point of ulceration, perhaps suppuration. The operation was readily consented to, and this large, unusual stone found, which had caused all her suffering, the patient making a good return to complete health.

Another condition in which the abdominal surgeon does not acquit himself so fortunately, is a case of many years' standing, with frequent attacks of pain accompanied by jaundice, light, clay-colored stools, and all the clinical symptoms present. All medical efforts failing, an operation, after consultation, is decided upon and done; the gall-bladder is found contracted to a mere sinus, and stenosis of the common duct, with many adhesions, yet no gall-stone found. The adhesions are loosened, and an attempt made to pass a probe not successful. The patient finally made a good recovery. It is not fair to assume, that, had this patient had an early operation, when medical treatment seemed to avail not and when the gall-bladder had not yet contracted, probably containing a number of gall-stones (some of which had been observed in the stools), she would have had a more speedy recovery, saving three or more years of pretty continuous suffering.

Is not this latter an illustrative case in which it seems wise for the general practitioner and abdominal surgeon to unite early their skill for the cure of the patient? When I speak of the united efforts of the general practitioner and the abdominal surgeon, I mean an honest, thorough looking over the case jointly. I do not mean to be understood that when the former has sent for the latter as an operating surgeon, he must necessarily go ahead and

operate. Medicine plays some strange freaks in relieving surgery of an operation now and then.

A patient, under judicious care of an able practitioner, gives all the symptoms of obstruction of the common duct, yet fails to respond to a line of treatment most thorough and complete. He is already in a condition of cholæmia, presents here and there over his body appearances indicating the approach of ecchymotic spots, so dangerous from the standpoint of surgical interference. It is decided by the family physician and friends of the patient to send for the surgeon who has operated in such cases. A careful going over the history of the case and a thorough consultation reveal the fact that a few remedies have not yet been made use of, and large doses of olive-oil are given for a few days, followed by the free use of succinate of iron, and the continued use of Sprudell salts. Within a week the pain ceased, and the washing of the free evacuations from the bowels presents this genuine gall-stone, which I here exhibit. It will be observed this stone presents no facets. This man had suffered for many years from gall-stone colic. He has now for two years or more been absolutely well—that is, since the olive-oil treatment.

That the common duct will dilate to a greater extent than has ordinarily been believed, I think is capable of demonstration from the specimen I next present.

This case was known to have had a number of attacks of biliary colic, dying, however, of another disease. An autopsy was held, and the specimen brought me a few hours after death. There were in the common duct, two of the gall-stones here seen, while all the others, with the exception of the largest one, by a little pressure passed readily into the duodenum. Of course some allowance must be made for post-mortem relaxation.

Let me inquire whether it is true that all general practitioners realize fully the serious dangers that are associated with attacks of biliary colic.

Those of us who, in days past, have held many autopsies, know too well of sudden deaths accompanying gall-stone colic, in which the shock to the nerve centres, from the great pain, was such as to turn the balance fatally against the patient.

Look at this specimen in the case of a man who was thought by the attending physicians likely to recover, as he had done in other well-known attacks of jaundice and severe suffering. It was known that he had passed gall-stones before, but now medical treatment and nature failed, and in a severe paroxysm he passed into a condition of collapse and died.

This gall-bladder, filled as it is with gall-stones, presents a most perfect specimen to illustrate what a grand chance was here offered the abdominal surgeon to perform cholecystotomy, with every prospect of his patient making a thorough and complete recovery, had the general practitioner joined hands with him in the treatment of the case.

Almost the same may be said of the other like specimens that accompany this. I regret that these three specimens were at the autopsies removed somewhat hurriedly, but at the time they showed clearly stenosis of the cystic duct.

Medical treatment has unquestionably done good in such cases by a carefully regulated diet, by an application of suitable therapeutics, and without dispute has helped the patient through successfully, but, again, on the other hand, is it not equally true that valuable lives have been lost, and the years of man lessened, in consequence of the too prolonged effort to treat that kind of a case which has been diagnosed and is now fully recognized as one of gall-stone impaction? To have the operation delayed, delayed until such adhesions and malposition of important organs have resulted as to greatly embarrass the operator at the time, and to interfere seriously with a favorable percentage of recoveries, is not keeping abreast of the work the abdominal surgeon is capable of doing.

These are to be classed with the cases that have suffered repeated attacks until a lodgment in the common duct is the inevitable result, a well-defined stenosis has formed in such a way that we have a sacculated condition of the duct or ducts, a contracted gall-bladder upon one or more stones—cases which, had they been thoroughly understood by the general practitioner, the abdominal surgeon called in early, and the operation done promptly, would certainly have resulted in a smaller rate of mortalities, a lessening of much suffering, a prolongation of life. I am sure that, while the general practitioner is justified at all times, so far as it is reasonable, to restrain and keep his patient from the operating table, that we have, on this subject of gall-bladder surgery, to make an advance in the direction of more early operation. It is necessary that the general practitioner should come more in touch with the surgeon, in the prompt, early, and frequent examination of these patients. The responsibility rests largely with the family physician.

Let me again illustrate: We have here a specimen. This patient had attacks of biliary colic two years previous to the time of his death. He was given a good, careful course of treatment, thoroughly intelligent in every way. He had his periods of being jaundiced, then would evidently get relief, remain well for a month or two, when the attacks of colic would return. For ten months previous to his coming under my notice the jaundice did not disappear. He lost greatly in flesh, and when thoroughly cholæmic, with numerous ecchymotic spots, it was suggested to him that an operation might be done for his relief, upon which suggestion he acted quickly. In doing the operation, I found just that kind of embarrassment met with in these cases, of nearly obliterated gall-bladder, *i. e.*, the gall-stone occupying the common duct, producing the obstruction, a full liver, many adhesions,—a difficult operation in every respect, but which was finally accom-

plished, working from the cystic duct downward, removing the calculus, and being able to pass a probe through into the duodenum. It was necessary to place in a glass drainage tube, and pack around with iodoform gauze. He made a good recovery from the operation, having movements of the bowels within twenty-four hours afterward, and the following twenty-four hours the stools gave evidence of containing bile; but he died from absolute exhaustion on the fourth day. No peritonitis or other immediate cause of death was present.

Had this case been reached earlier, the results might have been more creditable to surgery.

Among the cases of gall-bladder surgery are those in which the surgeon should be guarded as to the possible results in the formation of a fistulous opening, concerning which it is our duty to explain more fully to the family physician and to the friends. I am sure I have seen keen disappointment resulting in some of these cases where the continued discharge of bile became a source of annoyance, and the surgeon criticised somewhat in not being able to close the fistulous tract as early as had been promised. Life will be prolonged, however, even in these very few cases, where it becomes impossible to close the fistula. Better have a fistulous opening than complete obstruction of the common duct, with no outflow.

There should come to the abdominal surgeon of to-day a great degree of comfort and pleasure as he reviews his field of work, and realizes with what complacency it is possible to make a diagnosis, and to operate, in cases of cysts of the pancreas, desmoid tumors of the abdominal walls, and all of the rarer conditions such as are now met with in and about the abdominal cavity, and which are so amenable to treatment. But these conditions are such, as I have said before, that the general practitioner has ample time for securing expert assistance to aid in a proper diagnosis.

Among this group may be classed all the conditions such as tumors, etc., that develop slow obstruction of the bowels, but not to the conditions that produce acute, quick obstruction. Here a prompt, early joining of forces by the abdominal surgeon and general practitioner is imperative.

In connection with this division of abdominal work, there is that which claims our most earnest, careful attention, a subject that is not yet thoroughly mastered; a question that involves more acute diagnostic skill, a more careful compilation of the results of different operators and presentation of facts, in which the general practitioner must join, and that is the subject of appendicitis.

From my observation of vital statistics, causes of death, etc., I do not notice so much the use of that indefinite term, "inflammation of the bowels," as I do the diagnosis ventured, "obstruction of the bowels, cause not well defined."

I believe that the gentlemen who are here present will agree with me in congratulating ourselves as a profession that we are no longer to be distressed by the term "peri-typhlitis," and other similar expressions, as indicating some form of trouble with the appendix, and it is just at this point I feel keenly the inconsistency of my presence, for I know that I am in the midst of men who have reflected much credit upon American surgery, and who have distinguished themselves as authorities in this branch of abdominal work. However, it may not be so entirely out of place, as a sentinel coming from the outer line of pickets, to present a report such as one collects in meeting these cases, in a consultation practice spread over a section of country that embraces several villages, towns, and smaller cities. I have heard with great interest in the near past, one good, clear-headed practitioner say that he had treated ten cases of appendicitis, with nine recoveries, and but one death, the last being a case of

acute perforation, the patient dying within two or three days of the attack; the former cases embracing those in which abscesses had formed, a cherry stone, in one case, escaping from a sinus in the lumbar region; another case, a pumpkin seed escaping with discharge of pus through the rectum; another of apple seeds through an opening above Poupart's ligament; and yet he felt contented that his cases had escaped an operation, and had recovered so well. And then, on the other hand, I have heard another equally good practitioner say that he had, within two years, seen eleven cases of appendicitis, in one or two of which he had operated, but that ten out of the eleven had died of septic peritonitis, none of them living longer than six or seven days.

Now here are two reports from thoroughly reliable gentlemen in our profession, men whom we respect, men who are honorable members of the societies with which they are connected. One has but to take a section of his consultation work and gather much that presents points of intense interest, and food for careful, professional thought and study in this field of surgery.

July 29th, 1893, I received a telegram from a physician in a neighboring village, a man careful in the examination of his cases, and whose diagnoses are generally correct, requesting me to come prepared to operate for appendicitis. The patient was a lady, aged forty, resident of this city, married fifteen years, having one child twelve years of age. Four days previously she thought she had contracted cold while taking a bath at the Springs, but the symptoms from day to day pointed to trouble in the right inguinal region, temperature gradually increasing, reaching 103°, and her pulse 120 the night I saw her. She had had what was supposed to be inflammation of the bowels when fifteen years of age, and she remembers the doctor at that time asking her many questions as to whether she had been eating fruit containing seeds, etc., he evidently suspecting some trouble with the appendix. She was at that time living in Germany. After being sick two

months she ultimately recovered. Ten years ago she suffered from another somewhat similar attack, but was told by her attending physician that the difficulty was probably connected with the right ovary. When I saw her, I had but to confirm the diagnosis of the physician who sent for me, and that we would probably operate early the next morning. During the night, her family physician arrived, a very intelligent practitioner, and we saw the case on the early morning of July 30. She had passed a more comfortable night, one or two doses of morphine had been administered, her temperature was 100⅘°, pulse 88, and in many ways she was feeling much better. Her husband had arrived, and was exceedingly anxious, if possible, to postpone the operation. An enema given had resulted in a fairly good movement of the bowels, with considerable gas, and it was thought well to wait until the following day. Ice was applied with much comfort,—no anodynes, but a cautious line of treatment continued. I can only say that this case (a case, undoubtedly, of recurring appendicitis) made a gradual recovery,—a proper case now for operation, one that we feel almost sure will recover from an intermediate operation, which is undoubtedly becoming the proper line of treatment for all such cases. A case dangerous to be left to go on until another attack.

On Monday, July 31st, a call was received from an opposite direction to see a student, aged nineteen, a splendid specimen of physical development, a very intelligent person. On the 25th, he had made a nine-mile ride on his bicycle up the mountain side, and thinks that he ate a quart or more of black and red raspberries. This was on Tuesday. On Thursday he began to complain of some pain in his abdomen, believing that it was like an attack he had had at the seashore, and called then bilious colic. Similar remedies were made use of, but without benefit. He gradually grew worse Friday, and on Saturday vomited some. He vomited much worse on Sunday, a greenish, dark-looking fluid, distinctly mixed with blood. When I saw him at five p. m., Monday, his temperature was 102°, pulse 140, very little distention of the bowels, and he had a fairly liquid movement of the same while I was examining him. He had passed, with some of the

enemas that had been made use of, some berry seeds. He was delirious, and it was difficult to make out the second sound of the heart; was vomiting a greenish-looking fluid with some blood. Mutual diagnosis, acute enteritis, with possible perforation of the appendix. An operation not deemed advisable, as he was already becoming moribund, and died fifteen hours afterward.

The autopsy revealed a very excessive enteritis, the cæcum in several places being ulcerated to the point of perforation; the appendix contained several seeds, and at one point perforation was nearly complete, but the contents had not yet escaped into the peritoneal cavity. Probably a case of acute appendicitis. Possibly an early operation might have done some good.

August 7th, I responded to a telegram asking me to see the case of a man, aged forty-two, a machinist by occupation, who, ten years before, had had an attack of appendicitis with peritonitis. Two days previously to the time I saw him, after doing some heavy lifting, he was seized with severe pain in the right side. Later, the case was recognized as one of appendicitis by his family physician, who recommended an immediate operation. I found the patient's pulse 90, temperature a little more than 100°, he feeling a little better than he had forty-eight hours previously. There was no doubt about the appendix being implicated, and that the general practitioner was right in every respect. An operation was absolutely refused by his wife, this opinion being sustained by an older physician. This patient is not yet well, no abscess has pointed, no operation has been done, but the case is one unquestionably for intermediate removal of his appendix.

Monday, August 7th, a strong, healthy boy, aged sixteen, a farmer's son, developed gradually a pain that became very severe toward night, and located in the right inguinal region. The only history, in any deviation from health, was a supposed strain of some two weeks before. His bowels now became constipated, and he vomited some. He was seen by a very excellent, astute, keen practitioner, who, although advanced in life, is thoroughly alive to all modern diagnostic skill in reference to bowel trouble. The boy's temperature had gradually increased, and

on Sunday, August 13th, was 102°, with a pulse of 100. There was marked tenderness in the right inguinal region, and evidence of appendicitis well defined. I saw him on Tuesday, August 15th. The night before, he had had a free movement of the bowels, his temperature was normal and his pulse 80. Soreness and tenderness were confined strictly to the appendix, where an enlargement could be made out. There was such marked improvement in his case that it was thought best to wait, and not go on with an immediate operation. This young man has made a slow but continued convalescence, no abscess forming, and is now apparently well. Is this now a proper case for operation?

August 10th, a case presented of a lady, aged thirty-five, a school-teacher by occupation, treated when one year of age for what was believed to be inflammation of the bowels. Began menstruation at thirteen, never regular, and always suffering more or less. From November, 1892, until the time I saw her, she had four distinct attacks with all the symptoms indicating appendicitis. Her family physician, a most excellent gentleman of sixty, who had attended her in these last attacks, did not believe in an operation for appendicitis, and advised her to the contrary. She was emaciated, was unable to go on with her occupation, and had just recovered from her last attack when I saw her. I advised an immediate operation, to which she consented. I found many adhesions, the appendix doubled upon itself, shortened, and difficult to remove, which I here exhibit. She made an excellent recovery, has gained much in flesh, has had no return of her unpleasant symptoms, and is improving in every way.

A healthy, strong young lady, aged nineteen, had indulged quite freely the previous week in the eating of grapes. On the evening of September 15th, Friday, she was taken with severe pain in the epigastric region, after having exposed herself somewhat by sitting upon the cold ground; was given a seidlitz powder that night by her mother, which she soon vomited. She was seen Saturday by her family physician, a clear-headed, shrewd practitioner, who gave her some small doses of calomel

and hypodermic injection of morphine. She had two or three movements of the bowels, and was easier Sunday p. m., the 17th, when I saw her with her physician. She complained of pain in the region of the appendix on deep pressure, and I had but to confirm the doctor's opinion of appendicitis. I saw her early the next day, the 18th; her temperature was 102°, pulse 120, with evidences of suppurative appendicitis without doubt. She was removed to the hospital at once, and an operation done. There was much effusion of serum in and about the appendix, which had extended also into the cavity of the pelvis, lymph exudate thrown out, holding coils of the small intestines, pus forming. The appendix was found in a gangrenous condition, several grape seeds present. The peritoneal cavity and the pelvis were thoroughly washed out with hot water containing salt and alcohol, long glass drainage tube introduced into the cavity of the pelvis, immediate neighborhood of the stump of the appendix and cæcum packed with iodoform gauze as drainage, which was changed on the third day, the temperature showing a slight rise. The packing was continued, and the drainage tube left in for nearly a week, patient making an uninterrupted recovery.

A young man, aged twenty-five, comparatively well, was standing at his desk at ten a. m., Saturday, November 19th. While paying off his employees, was seized with a sudden, severe pain, which compelled him to cease work, though within an hour he walked nearly a mile to his home. Vomited some. Seen at once by his family physician, who was obliged to give him hypodermics of morphine to relieve the pain, but who, from the symptoms, made a diagnosis of perforative appendicitis. The next day, when I saw the case with the doctor at two p. m., his pulse was rapid, he had all the local symptoms of acute appendicitis; his heart's action and capillary circulation were such that an effort was made to rally him somewhat, although useless. The operation was done early Monday a. m., though the case was moribund at the time, and he lived but a few hours afterward. The autopsy showed perforative appendicitis, with a most malignant septic peritonitis.

I might cite many other cases occurring in the same space of time, but these I have selected as a section of the circle pertaining to the abdominal work of the consultant surgeon's experience, and we have here an illustration that brings out the points I would like to hear discussed.

There is no question in the minds of operating surgeons that in appendicitis an abscess does form, and nature will make an opening, and the cases do recover; but a large number fail to do so, because an operation is not done sufficiently soon, and because the general practitioner has not yet reached the point of believing that operations in these cases save life.

Then, again, is it not true that we are reaching the point where we must admit that the intermediate operation for relapsing appendicitis is an established procedure in surgery? But are we yet on solid ground in reference to our cases of acute, perforative appendicitis? Is the general practitioner yet in a condition to diagnose such cases, able to make a prompt, early diagnosis, and have we any array of statistics that will warrant the abdominal surgeon in making an immediate operation, treating these cases as we would bullet wounds of the intestines? When we consider the important part acted by certain micro-organisms, such as the bacillus coli communis, when we realize how rapidly septic peritonitis takes place in the vast majority of these cases, we slip, as it were, to ask ourselves the question, Have we as yet had any of these cases recover, following ever so early an operation? This is the branch of appendicitis, it seems to me, in which we have much to consider. Advances have yet to be made. Are the middle-aged and elderly members of the family of general practitioners willing to allow the operation done so promptly, even though they have made the diagnosis? Have we yet sufficient statistics that we can hold out any encouragement to the friends of the patients—have we

anything yet, as abdominal surgeons, to encourage ourselves in the operation, in these dreadfully acute cases? I mean these intensely acute septic cases of perforating appendicitis. There is, in these cases, little warning—the wound is made; the contents of the intestinal tract are in the cavity of the peritoneum with the first exclamation of pain.

The general practitioner is, without doubt, making a more early and correct diagnosis in these cases. The sad case I have just reported was diagnosed at once by the middle-aged, able practitioner in charge, and could he have reached a surgeon, and had his own way, he would have had an operation that night, so certain was he of his diagnosis.

There is one condition that is yet to the general practitioner a somewhat unexplored field. Heretofore—that is, up to within a few years—where the diagnosis has been made, they have looked upon a certain class of cases as almost hopeless. I refer more especially to patients suffering from tubercular peritonitis. Abdominal surgery has done much, very much, for these cases.

While the pathology is not yet absolutely settled, we know from clinical experience that drainage cures a large proportion of these cases and can be reached in no other way. I think they, too, belong to a classification in which the general practitioner and surgeon should come more in contact with each other in their examination than has been the case in the past.

In pelvic surgery, that which comes under the observation and control of the abdominal surgeon, there is a great responsibility resting upon the general practitioner in keeping in close touch with his consultant.

It is now more than thirty years since the French pathologists, Bernitz and Goupil, gave to the profession the results of their pathological investigations relating to pus tubes and suppuration of the uterine appendages, yet

observe how slow we have been as operating surgeons in making use of their studies. One feels as though our dear old professor had been lecturing upon this subject; but now we know that there is no portion of the abdominal cavity that requires better diagnostic skill, better judgment as to operation or no operation, than the pathological conditions that are to be found within the pelvis, but even these difficult problems are being solved and the profession at large greatly benefited by the earnest, untiring work of the men who made this their special labor.

DISCUSSION.

DR. ROBERT F. WEIR.—The paper with which Dr. Vanderveer has favored us is of great interest both to the general practitioner and to the surgeon. It starts out with the idea that early operative interference in acute abdominal surgery and particularly in cases of appendicitis should be advocated more and more. This last is a point in connection with appendicitis with which the New York practitioner has grown tolerably familiar; I mean not only the practitioner of this city, but of the surrounding country as well. This familiarity is principally due to the large experience that emanates from such a great city as this, with its many hospitals and surgeons, and with its acquired knowledge being widely disseminated through the medium of the medical journals, the medical societies, and particularly through the patients who have recovered from the surgical treatment of their affliction. The subject is an important one, and well worthy of the repeated and elaborate discussions it has given rise to. Notwithstanding this, at a recent meeting held in this city, the president of the society choked off further discussion of a case of appendicitis with the statement that the subject had been so frequently discussed that it was useless to prolong it. I do not agree with him. The important question for the general practitioner, When shall the operation be performed? has not yet been solved. I am of the opinion—and I think others will corroborate my statement—that there are general practitioners who are more surgical than the surgeon. One physician of great and proper emi-

nence suggests from its known pathology—of perforation, gangrene, or distention—that these patients should be submitted, in every instance, to a laparotomy at the very beginning of their trouble, as soon as the diagnosis is certain, whether the symptoms are mild or severe. We surgeons cannot take that position, because the general practitioner inevitably and convincingly cites many cases in which recovery took place without operation. We have all seen such statistics, and must bow to them.

I take it that the chief point in Dr. Vanderveer's paper is the early coming together of the physician and surgeon, in order to obviate too late operative interference. This is of weight, not alone in appendicitis, but in gall-stone obstruction, gall-bladder empyema and perforation, gastro and intestinal perforation, and in sundry other conditions of the abdominal cavity. But in the right *iliac fossa* much yet is to be learned, for it is an unfinished subject. It is in this corner that surgery has recently achieved its greatest triumph.

The question that is in my mind, and the question which the general practitioner wants to have answered is, How do we, as surgeons, know when to operate, or when not to operate, in these early cases of appendicitis? He knows as well as the surgeon does that an attack beginning, perhaps, with unusual severity, may in the course of twenty-four hours subside, and revert to the medical domain. He also knows that an attack beginning very insidiously may, within the course of twenty-four hours, give rise to symptoms of great severity. He knows, too, that surgeons differ in their opinions as to the proper time for operation. If he is a close observer, and of an inquiring turn of mind, he will ask, Why does the surgeon advise waiting in one case, while the same surgeon, in another case, may insist on immediate operation? A patient, for instance, is seen in consultation by the physician and surgeon, and they have agreed to wait, say, twenty-four hours. The next day the patient looks improved; his pulse is better; his belly is less tense, even though no opium was administered. In spite of this apparent improvement, the surgeon, if he is experienced, perhaps fastens upon something and says, "We cannot wait any longer." The knowledge that comes from experience gives the surgeon something that enables him

to say that the patient, although apparently doing well, is not out of danger. One man depends largely upon the respiration; another will lay great stress on the pulse. We have not yet a concurrence of symptoms that we can clearly present to the general practitioner, so that he can judge for himself whether an operation should or should not be performed. Still, an expression of opinion from half a dozen or a dozen experienced surgeons on this point would, I think, prove of great value to the profession.

I would like to indicate what my own methods are in arriving at a determination in these early and doubtful cases. It is in the early stage that the difficulty arises. Out of a series of 120 cases of appendicitis operated on in the early stage, *i. e.*, within seventy-two hours of the inception of the disease, before the extension of the inflammatory process, the mortality was five per cent. Just contrast that with the rate of mortality following the performance of the operation in the so-called cases of relapsing appendicitis; in those cases the mortality runs from two to three per cent., perhaps a little bit higher, because the bad cases are not always reported. So it is seen that the early operation in itself presents a most favorable result.

Individually, in the ordinary cases of appendicitis, as we frequently see them, I am willing to wait for twenty-four or forty-eight hours, excepting in those that begin very severely. After waiting for this period, unless there is a coincident amelioration of all the important symptoms, I advise operative interference. This coincidence of improvement in all the symptoms, is the point I lay stress upon. If I only find an improvement in one of the three or four dangerous symptoms, namely, the pulse, the temperature, the pain, the rigid abdominal walls, etc., I prefer to advise the risk of an operation, which I consider infinitely less than that connected with the progress of the disease. Since my adoption of this rule, some three years since, I have steadily been impressed by experience of its great value, and therefore venture to urge it upon the attention of the profession.

DR. JOSEPH D. BRYANT.—It has been my fortune—or perhaps ill fortune—to have heard a statement of the same tenor as that referred to by Dr. Weir, to the effect that

when the subject of appendicitis comes up for discussion, physicians are apt to feel that it has been worn threadbare. I have given it considerable attention, particularly from an anatomical standpoint, including the condition of the appendix and its contents in those who have died from other causes than appendicitis. I here wish to make the confession that there is no class of cases which I am asked to see in consultation, about which I feel so much doubt and fear as those of appendicitis. I agree with Dr. Weir, that this question is far from being settled. I do not propose to go over the symptoms usually met with in this disease. They have already been sufficiently considered by Dr. Weir. In the treatment of these patients, notwithstanding the fact that I have conscientiously followed the work and writings of others, and have tried to formulate rules for my own guidance, I have met with serious and almost heartrending disappointments. Patients who I thought would recover have suddenly developed genuine peritonitis and died; in others, who I believed required an operation, I found, when the abdomen was opened, evidences of resolution which, no doubt, would have ended in recovery without interference. I do not know, and cannot say exactly as to all the cases that demand operation, and those in which an operation may be safely postponed or abandoned. Such must be the case, from a logical consideration of the subject. No man can say positively that the vermiform appendix is gangrenous, although the symptoms may indicate it. No man can say positively that perforation has taken place, although the symptoms may indicate it. Cases have occurred in this city and elsewhere, in which eminent surgeons, after prolonged consultations and examinations, and a careful consideration of all the conclusions, have operated and found no appendicitis. Whenever a person tells me that Surgeon So-and-so operated on a patient for appendicitis and found nothing, I am inclined to regard it as an evidence of advance rather than a lack of perception or knowledge of the subject.

In 150 cases of autopsy made at the Bellevue deadhouse, I have had an examination made of the appendix vermiformis, with the object in view of ascertaining its exact contents and in how many instances it was diseased.

In not one single instance was the traditional grape-seed or bird-shot present; nor, in fact, was anything present excepting inspissated feces or muco-pus.

The paper that has been presented to us to-night treats largely of the relations between the general practitioner and the surgeon. How shall the surgeon educate or instruct the general practitioner as to the symptomatology of appendicitis, so that he may act promptly and in unison with the surgeon to benefit his patient? I know of but one way in which that can be done. I am well aware of the fact, and so are other surgeons, that when we come together in societies to discuss appendicitis, or the radical cure of hernia, rarely, indeed, a physician attends. I am also aware that when a discussion takes place regarding the treatment of typhoid fever, rarely a surgeon makes his appearance. In my judgment, whenever a subject of common knowledge comes up for discussion—a subject that may require the services of both physician and surgeon—the attendance of both should be promptly demanded, and as promptly given. Whenever a physician calls a surgeon in consultation, and an operation is performed, the physician should be present at the operation, for the reason that he is thoroughly acquainted with the symptoms of the case, and he will be able to judge of their importance by the pathological processes which the operation reveals. And finally, in order that both the physician and the surgeon may learn, they should both be present at the autopsy. I know of no other way in which they can be brought together, and I know of no other way in which their concerted action will result in so much good.

I think you will pardon me for digressing for one moment from the subject under discussion, and referring to one of still greater importance, namely, malignant disease, in the treatment of which so much procrastination takes place. This subject is so important that we should begin —perhaps in the primary schools—to impress upon the minds of the young that whenever they meet with a growth of the human body that is abnormal, it is entitled to respectful consideration.

DR. SENECA D. POWELL.—I am glad Dr. Bryant made such an honest confession in his address, because it

relieves me to find that I am in such good company. There are one or two points that I wish to speak of, and I will lay most emphasis upon one which I consider the vital point in connection with abdominal surgery, and that is, diagnosis. I am not referring particularly to appendicitis, but to all growths or conditions of the abdominal cavity for the relief of which the surgeon is called in. While it seems an easy matter to make a diagnosis of gall-stones, yet I have met with several such cases in which I made a woeful mistake. One such case I have in mind now. The tumor was down in the neighborhood of "McBurney's point." Its appearance was coincident with the onset of acute symptoms, and grew rapidly. The patient was under the care of a good man, a general practitioner, who pronounced it a case of appendicitis. I was asked to see the patient, and corroborated his diagnosis. An operation was performed, and I found a distended gall bladder, containing two large gall-stones.

In reference to recurring attacks of appendicitis and the intermediate operation, I believe it is better to wait in such cases until three or four attacks have passed before advising operation. Such attacks, according to my experience, are usually sub-acute in character, the patient at no time having very severe constitutional symptoms. In such cases, the question arises whether an operation is advisable. If you can feel a well-marked growth, or tumor, and find evidence of an acute inflammation, or special evidence of suppuration, then an operation is justifiable. I do not think that those cases of appendicitis which Dr. Vanderveer spoke of as resembling gunshot wounds of the intestine should be included in the range of this discussion, which relates more especially to doubtful cases. The diagnosis in the former is beyond doubt, and they invariably demand immediate operation, if the patient can survive it.

The question of whether an operation is necessary or not, must be determined by the growth, or by the absence of a growth, and by the symptoms. When once we decide that an operation is demanded, then the exact pathological process—whether it is an appendicitis, or a suppurative process of the ovary, or a distended gall-bladder, is best determined after the abdominal cavity is opened.

DR. MCBURNEY.—I desire to congratulate Dr. Vanderveer on the admirable paper he has read to us, and to refer especially to what he has said to us in regard to the relationship between the general practitioner and the surgeon, or specialist. It might be inferred, from some of the articles which I have read, that the physician is afraid of the surgeon, and considers it a very grave matter to ask his opinion in regard to a patient. The general tendency of the practitioner is to avoid surgery, and this tendency is a very natural one. The practitioner desires to cure by his own methods, and it is a very serious thing for him to suddenly have to face an emergency of the greatest importance, to have it suggested to him that a grave surgical operation is demanded in the treatment of his case. Nevertheless, it is certain that sometimes these emergencies must be met, and that in some cases the surgeon's work is essential to the patient's recovery.

The surgeon also loves to cure by his methods. They are direct, they are thorough, and often successful. In cases of appendicitis, the experience of the surgeon may be of much value, and there is no reason why the medical practitioner should not have the advantage of it.

An attack of appendicitis is often sudden and unexpected in its onset, and gives rise to severe symptoms, and the question, whether an operation is necessary or not, must often be answered without delay. As regards the exact time to operate in appendicitis, I have been asked that question many times, and I wish that I could answer it by giving some clearly-defined rules. But, neither in regard to other diseases nor in regard to cases of appendicitis, can definite rules, which apply well to many different cases, be laid down. But one who has had a considerable experience with any particular disease, whether it be pneumonia, or typhoid fever, or appendicitis, will be able, if you give him a history of the case, and then allow him to examine the patient, to tell you, in the majority of instances, how far the disease has gone, and what it is necessary to do.

It seems to me, then, that in all cases which, while apparently medical, may at any moment require surgical treatment, an early combination of medical and surgical experience is desirable.

DR. VANDERVEER.—(Closing the discussion.) I desire to thank the gentlemen who have been so kind as to present their views regarding this question. I fully agree with Dr. Weir that the subject is still an unsettled one. The all-important question is, When shall we operate? You are sent for to examine a case of appendicitis. The pulse, the temperature, the rectal examination—everything points to a favorable condition. The patient is doing as well as can be expected. You decide to operate, and in your attempt to break up adhesions and remove the appendix, you may perhaps set up an inflammation which results in the patient's death, and the general practitioner will feel somewhat disappointed. Your experience must be your guide, and tell you when to operate and when to let the patient alone. The pulse, the temperature, and the condition of the abdomen will tell you that you have a serious lesion to deal with. I agree with Dr. Weir that all the symptoms must be taken into consideration before coming to a decision. Do not depend simply on one or two of the symptoms.

All of us, I think, like Dr. Bryant, have at one time or another opened the abdominal cavity and felt that we had done better to have left it alone. Perhaps the septic peritonitis would not have occurred, had we left it alone. Dr. Powell has referred to errors in diagnosis. Such will occur, as no one is infallible. I do not think there is any field of surgery in which the surgeon must be prepared for greater surprises than in the abdomen. Lesions in this region are often very difficult to locate.

Dr. McBurney in his remarks touched on some very strong points regarding the relationship between the general practitioner and the surgeon, and I fully coincide with his views.

OBSERVATIONS ON EXCESSIVE INTESTINAL PUTREFACTION.

BY C. A. HERTER, M. D.,

AND

E. E. SMITH, PH. D.

Attention was called, not long since, to the frequent occurrence of excessive intestinal putrefaction in epileptics,[1] and observations were brought forward to show that in some cases of epilepsy the occurrence of this condition appears at times to determine the epileptic seizure.

The facts brought to light in the study of this subject stimulated an inquiry into the occurrence and character of putrefactive processes in the intestines in clinical states other than epilepsy. This inquiry, which has been carried on during the past three years, makes it clear that deviations from the normal character and degree of putrefaction in the contents of the intestine are met in association with a variety of morbid states. The preparation of the present paper was prompted by the belief that disturbances of this nature have received less attention than from their practical importance (in both diagnosis and treatment) they seem to deserve. The following has been written with the object of presenting, first, the evidence we possess as to the existence of putrefactive processes in the intestine; second, certain facts as to the clinical conditions in which putrefaction occurs in excess; and, third, the principles that should enter into the management of cases where such excess is present.

During many years the belief has been gaining ground

[1] Observations on the Etiology of Idiopathic Epilepsy, by C. A. Herter, M. D., and E. E. Smith, Ph. D. *New York Medical Journal*, September 3, 1892.

that various conditions of disease may arise in man through the absorption from the gastro-intestinal tract of toxic substances formed within it. For example, clinical evidence has suggested that some kinds of headache, most cases of urticaria, the prostration and fever associated with certain attacks of diarrhœa, and even many cases of simple anæmia, may depend upon the entrance of toxic materials into the circulation.

The well-known immediate symptoms of constipation bear out this idea, and perhaps no more striking clinical evidence can be found than the non-septic post-partum rise in temperature familiar to every obstetrician, which rapidly subsides after a thorough emptying of the bowel. And yet, while a good deal might be said in favor of the toxæmic origin of these and other states, it must be owned that the arguments for this view rest chiefly upon clinical considerations, and are at present unsupported by accurate knowledge of the nature of the pathological conditions involved. A partial exception to this statement may be made in the case of processes concerned in the putrefaction of proteid substances in the intestine.

It has long been known that a variety of aromatic substances, including indol, phenol, cresol, catechol, etc., are produced in consequence of the action of anaërobic bacteria upon proteids, and it has been known that this process of putrefactive decomposition occurs normally to some extent in the human intestine when nitrogenous food is introduced. In 1880[1] a very important fact was brought to

[1] At present, little is known of the special bacteria that give rise to the various products of putrefaction that are met in health and disease. The following facts relating to this subject have been ascertained:

Numerous microörganisms are capable of inducing putrefaction of albuminous substances. In the initial stages of the process, aërobic and facultative anaërobic bacteria usually play the most important part, while in active putrefaction they are supplemented or perhaps replaced very largely in the latter stages by strict anaërobics, organisms which have received comparatively little study. Among the facultative anaërobic bacteria, probably the most important and ordinary exciters of putrefaction are the bacilli of the proteus group; they

light by Baumann—namely, that these various aromatic bodies, however formed within the intestine, are at last eliminated from the body (if absorbed) through the urine, almost exclusively in combination with sulphuric acid, in the form of ethereal sulphates.

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are of common occurrence, and possess to a high degree the power to produce putrefactive decomposition. *Proteus vulgaris* (Hauser, *Ueber Fäulnissbakterien*, Leipsic, 1885) and *proteus mirabilis*, the commonest species, are liquefying motile bacilli and grow rapidly in the usual culture media at ordinary temperature. Other putrefactive bacteria are *bacillus coprogenes fœtidus* (Schattelius, *Der Rothlauf der Schweine*, Wiesbaden, 1885), found in the intestine of a diseased pig; *micrococcus fœtidus* (Klamann, *Allg. med. Centralzeitung*, 1887, p. 1344), from the posterior nares of a man; *bacillus pyogenes fœtidus* (Passet, *Untersuchungen über die eitrigen Phlegmone des Menschen*, Berlin, 1885), obtained from an abscess of the anus; *bacillus putrificus coli* (Bienstock, Ueber die Bakterien der Fäces, *Zeitschr. für klin. Med.*, 1884, Bd. viii), from adult fæces; *bacillus fluorescens liquefaciens* (Flügge, *Die Mikro-organismen*), a very common form occurring in putrid infusions; *bacillus coli communis* (Emmerich, *Deutsche med. Wochenschr.*, 1884, No. 50), found in the air, in putrefying infusions, in normal and abnormal fæces, and elsewhere; and *bacillus pyocyaneus* (Gessard, De la pyocyanine et de son microbe, *Thèse de Paris*, 1882,) occurring in green and blue pus, and probably a widely distributed saprophyte. *Bacillus cadaveris grandis* (Sternberg, *Report on the Etiology and Prevention of Yellow Fever*, Washington, 1891, p. 21) is a large bacillus obtained from the interior of dead animal matter in a state of putrefaction and belongs to the strict anaërobics.

When intestinal contents and normal fæces are examined, a very large number of microörganisms are to be found. Even the milk fæces of sucklings are no exception in this regard, since normally at least two varieties of bacilli are present (Escherich, *Die Darmbakterien des Säuglings*, 1886): *bacillus lactis aërogenes*, which is more abundant in the small intestine and produces an acetic-acid fermentation of milk sugar, and *bacillus coli communis*, most abundant in the large intestine, which, besides giving rise to an acetic-acid fermentation, occasions the formation of large quantities of lactic and formic acids. No true putrefaction occurs, however, since milk casein is not changed by these bacilli. In catarrhal enteritis the alvine discharges contain a much larger number of microörganisms, while in diarrhœa associated with toxic symptoms, one or more species of the proteus group appear to be constantly present (Booker, *Trans. of the Amer. Pædiatric Soc.*, vol. i, 1889, p. 198). Thus the bacterial condition of milk fæces is comparatively simple, and well illustrates the relation that exists between bacterial growth in the alimentary tract and some forms of disease. But the intestinal contents of adults with a mixed diet present a far more difficult problem. Here a much larger variety and number of microörganisms are normally present, producing changes in all the different classes of foods, including even putrid decomposition of proteids. Indeed, over fifty known species have been isolated from intestinal contents; and in addition to

There is reason to think that other acids than sulphuric acid may enter into combination with the aromatic bodies formed in the intestine, but notwithstanding this the ethereal sulphates are undoubtedly the form in which by far the greater part of these bodies is eliminated. The ethereal sulphates, therefore, become an index of the amount of putrefactive products absorbed from the intestine, and the determination of their amount in the urine is an indication whether the putrefactive processes are or are not excessive.

This fact is the basis of the present study of intestinal putrefaction, as well as of many previous investigations upon the same subject. It, therefore, becomes of the highest importance to inquire more carefully into the nature of the evidence on which this fundamental proposition rests.

Evidence that the Ethereal Sulphates of the urine are derived from the putrefaction of proteid substances in the

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these are many unidentified and perhaps unknown species found in normal and pathological conditions. The commonest form appears to be *bacillus coli communis*; probably certain strict anaërobics not yet carefully studied play an important part.

The particular products formed by albuminous putrefaction will depend on the nature and condition of the material, the conditions under which it is placed, and the particular microörganisms inducing the change. Toxic proteids are formed by bacterial growth under some conditions, both from living protoplasm and in contact with proteids not associated with living tissues; but they are not usually found in active putrefactive changes. The greatest resistance which is offered to albuminous decomposition is in the initial stages of the process, the peptonization of the proteid; and if this be already completely accomplished by other than bacterial ferments, as in pancreatic digestion, then the conditions are much more favorable for active bacterial growth with the characteristic formation of ptomaines and other putrefactive products. The nature of these is well illustrated by Bienstock's study of *bacillus putrificus coli*. In Cohn's nutrient solution, containing fibrin in suspension, into which small quantities of these bacilli were introduced, the following substances were produced—namely, peptone, ammonia, amine bases, fatty acids, amido fatty acids, tyrosine, phenol, paraoxyphenylpropionic acid, paraoxybenzoic acid, indol, skatol; further, when the bacilli were allowed to act on the individual products the lower series of decomposition products were produced; for example, tyrosine gave rise to paraoxybenzoic acid, the latter to phenol, etc., the final products being the simple gases, carbon dioxide, ammonia, methane, etc.

intestine.—We may now pass to the consideration of the evidence which proves that the ethereal sulphates of the urine are derived from the putrefaction of proteid substances within the intestine.

Sulphuric acid occurs in the urine in two combinations —first, as neutral salts of the alkalies, the so-called "preformed" sulphates; and, second, as ethereal or aromatic sulphates,[1] which are combinations of sulphuric acid or acid sulphate of potassium with various aromatic substances, particularly indol, phenol, and skatol, and to a less extent with cresol and catechol or pyrocatechin. The quantity of sulphuric acid in this combination, which is excreted daily by normal adult individuals, varies considerably, both in the same and in different persons. We have found that in most persons the daily output ranges between 0.12 gramme and 0.25 gramme.[2] The quantity excreted by the urine of course gives us no information as to the proportion in which the sulphuric acid is combined with indol, phenol, skatol, etc., since these individual and special combinations constitute no fixed proportion of the total. In most cases in health the combination with phenol, phenol-potassium sulphate, is the most abundant of the ethereal sulphates.

It is known that the aromatic substances are occasionally eliminated through the urine in combination with glycuronic acid, but this occurs in the urine in such small quantities that, except after the administration of certain drugs. it may be considered to be practically absent.

The various ethereal sulphates of the urine were at one time thought to be derived from the aromatic substances taken with the food, in much the same way that hippuric acid and its salts may originate. Some kinds of food

[1] They are also known as combined sulphates.

[2] These figures are based on a large number of personal observations. Von Noorden gives the same figures; but, as he points out, the amounts may exceed or fall below them without necessarily being pathological.

taken by man may furnish the material for the formation of ethereal sulphates in small amount, and under these circumstances this may explain the presence of a small part of the aromatic sulphates; but, as a rule, the food of man contains little of an aromatic nature, and it has been found that upon an ordinary diet the ethereal sulphates from this source amount to practically nothing.

This view of the origin of the ethereal sulphates had, therefore, to be abandoned. It was replaced by the theory of Salkowski,[1] who, as late as 1886, expressed the belief that one of the ways in which the ethereal sulphates arise is through their formation in the tissues of the body. The facts which seemed to give support to this view were as follows: Salkowski[2] found that in starving animals the urine contained large quantities of indoxyl-potassium sulphate (indican). Another observer, R. Van der Velden,[3] found that the ethereal sulphates were reduced only one half in a dog which had been kept without food for periods of five and six days. Further, Senator,[4] studying cases of chronic inanition in human beings, observed that in many instances the urine contained an abnormally large amount of a particular ethereal sulphate—indoxyl-potassium sulphate (indican). It is, of course, easy to see how these facts might be interpreted to support the theory of the formation of the aromatic sulphates outside the alimentary tract—that is, in the tissues themselves—but the classical observations of Baumann[5] were destined soon to put an entirely new aspect upon the matter. These observations were made upon a patient who for many weeks passed the

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[1]Ueber die Entstehung der aromatischen Substanzen im Thierkörper. *Zeitschr. f. physiolog. Chemie*, 1886-X-266.

[2]*Berichte der deutsch. chem. Gesellschaft*, Bd. ix, S. 408.

[3]Ueber die Ausscheidung der gepaarten Schwefelsäuren im Harn. Virchow's *Archiv*, 1872-LXX-343.

[4]*Ctrlbl. f. d. med. Wissensch.*, 1877.

[5]Die aromatischen Verbindungen im Harn und die Darmfäulniss. *Zeitsch. f. physiolog. Chemie*, 1886-X-126.

intestinal contents through a fistula in the upper portion of the small intestine. Baumann found that during the entire period in which the intestinal contents were diverted through the fistula there was a remarkable diminution in the amount of the ethereal sulphates in the urine, indol, phenol, and the aromatic oxyacids being present in mere traces. Just as soon as the fistula was closed, a pronounced increase occurred in the amount of the ethereal sulphates excreted by the urine. The interpretation very properly given these results was, that the occurrence of putrefaction in the contents of the intestine is the cause of the production of the ethereal sulphates, these substances appearing in the urine when the fæces are passed by the anus because of the opportunity then given the intestinal mucous membrane to absorb whatever putrefactive products are formed in the gut, but disappearing very largely when the intestine is emptied above by the artificial anus, because putrefaction of the intestinal contents is only just beginning at this stage of digestion, and the amount of the putrefactive products capable of being absorbed by the mucous membrane is very small. In the light thrown by these crucial observations of Baumann, some earlier studies made by Ewald[1] became significant. Ewald had in 1878 made observations upon a case of intestinal fistula resembling Baumann's, and had noticed that, so long as the chyme was discharged through the opening, the urine was free or nearly free from phenol and indol, but that these substances reappeared in normal or excessive amounts as soon as the fæces were passed *per anum*. A very satisfactory and interesting confirmation of the results of Baumann and Ewald, from another point of view, was furnished in 1888 by Kast and Baas.[2] Their observations were made upon a case of

[1] Ueber das Verhalten des Fistelsecrets und über Phenol-und Indican-Ausscheidung, bei einem an Anus præternaturalis leidenden Kranken. *Archiv für path. Anat.*, 1879-LXXV-409.

[2] Zur diagnost. Verwerthung der Aetherschwefelsäureausscheidung im Harn. *Münchener med. Woch.*, 1888, No. 4.

complete occlusion of the small intestine from cancer. The occlusion persisted twenty-three days, and during this time a large amount of the ethereal sulphates was regularly found in the urine. At the end of this period the stoppage was relieved by operation, and there at once followed a great diminution in the amount of aromatic sulphates excreted. We have ourselves observed a very large increase in the ethereal sulphates, and in indican in a case of occlusion of the small intestine in the jejunum from external pressure at the time when this stoppage was established. Pointing in the same direction are the results obtained by Baumann and Wassilieff,[1] who found that there was a complete disappearance, or at least a great reduction in the ethereal sulphates eliminated by starving animals, when the contents of the intestine were pretty well got rid of by the use of large doses of calomel for several days. Morax,[2] working a few years later, got practically the same results with calomel. He found, also, that very large doses of iodoform in dogs freed the urine temporarily from the presence of ethereal sulphates. All these results, reached as they have been by competent observers, furnish strong evidence that the tissues in health have nothing to do with the formation of aromatic sulphates, and the conclusion is justified that intestinal putrefaction is in health the sole source of their formation, barring only the trivial influence of food containing aromatic substances.

In connection with the view that the ethereal sulphates might be formed by the tissues, it is of interest to note that Müller[3] was unable to find any evidences of the presence of

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[1] *Zeitschr. f. physiolog. Chemie*, Bd. vi, S. 112.

[2] Bestimmung der Darmfäulniss durch Aetherschwefelsäure im Harn. *Zeitschr. f. physiolog. Chemie*, 1886–X–318.

[3] Indicanausscheidung durch den Harn bei Inanition. *Mittheil. d. Würzburger medic. Klin.*, 1888–XI–342. Ortweiler (*ibid.*, S. 153) found that in fever, increased destruction of tissue, there was no increase of indican; but this fact cannot properly be used as an argument against the formation of ethereal sulphates in the tissues.

these aromatic putrefactive products in the muscles and parenchymatous organs in health. He found, however, that the fæces of starving animals contained indol, and this fact was only an additional confirmation of the correctness of the view, that the presence of ethereal sulphates in the urine of starving animals does not constitute any evidence that their production occurs in the tissues, but is far better explained on the ground already mentioned. It should further be borne in mind that, as already mentioned, Kuhne[1] and Nencki have shown conclusively that indol is exclusively the product of the action of bacteria upon proteids. If it should be held that possibly the aromatic substances may be produced by the action of bacteria in normal tissues, the fact should be remembered that Meissner, Zahn, and Hauser[2] were unable to find bacteria in normal tissues. The *bacillus coli communis* has indeed been repeatedly found in spleens that were apparently normal, but only, we believe, where there has been ulceration of the intestinal mucous membrane.

The view that the ethereal sulphates arise as the result of intestinal putrefaction, is the only view which accords with observed facts. It does explain these facts fully, and is supported by a large amount of satisfactory experimental and clinical evidence. It may be added that, so far as we are aware, this explanation of the origin of the ethereal sulphates of the urine has been accepted by all modern writers upon chemical physiology and pathology.

We must note one important exception to the origin of the aromatic substances from the putrefaction of proteids in the intestine. The ethereal sulphates may be increased in consequence of putrefaction in other parts, especially where there is extensive suppuration without free drainage.

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[1] *Berichte d. deutschen chem. Ges.*, 1875-XVIII-206.

[2] *Berichte d. deutschen chem. Ges.*, 1875-VIII-336; also, *Ueber die Zersetzung der Gelatine und des Eiweisses bei der Fäulniss mit Pankreas*, Bern, 1876, S. 31.

Such an increase has been observed in abscess in various parts, in tuberculous lung cavities, and in cases of empyema. It is scarcely necessary to emphasize the importance and the difficulty of excluding intestinal sources of excess before concluding that an excess of the combined sulphates, or of any particular product of putrefaction, is due to a suppurative process.

The use of certain drugs of an aromatic nature may enormously increase the output of the ethereal sulphates, and may lead to serious error if the origin of the excess be not appreciated. Thus we have found a great increase from the use of salol in two individuals, the increase being referable to the elimination of the phenol of the drug[1]. The use of creosote must be attended with the same result. Von Jaksch[2] has recently called attention to a largely increased output of aromatic sulphates, noted by him in a patient who had been poisoned with benzosol, the increase being doubtless due to the combining of the drug with sulphuric acid.

How are we to decide whether, in a given case, the ethereal sulphates are in excess or not, whether we have or have not to deal with an example of excessive intestinal putrefaction? This is a question of the highest practical importance, for unless our criterion of excess be essentially accurate, there is danger of falling into grave error in the interpretation of results. As already stated, the quantity of sulphuric acid excreted daily in healthy as ethereal sulphates varies ordinarily between such wide limits as 0.12 gramme and 0.25 gramme.[3] But the quantity may be

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[1] In one case where the subject had been excreting about 0.200 gramme of ethereal sulphates daily on a mixed diet, the amount rose to 0.446 gramme and 0.662 gramme on ten grammes of salol three times a day, and to 0.933, 0.955 gramme, and 0.966 gramme, on twenty grammes of salol daily. The ratios were 3.7 and 1.7, and 0.6, 0.6, and 0.8 during the two periods respectively.

[2] Ein Fall von anscheinender Vergiftung mit Benzosol. *Berl. klin. Wochenschr.*, February 27, 1893.

[3] Calculated as $H\ SO_4$.

increased to 0.3 gramme or even 0.4 gramme without our being able to say from the quantity alone whether we are dealing with an excess, for the quantity of proteid food ingested may be very large, and we should expect that the opportunity for the activity of putrefactive bacteria would be greater in this case. On the other hand, the amount eliminated may lie between the figures mentioned, and thus be apparently normal. But the quantity of proteid food may have been unusually small, so small and of such a kind (milk, meat) as to be accompanied in perfectly normal digestion by the elimination of even less than 0.12 gramme in the day. The mere amount, therefore, does not afford a thoroughly satisfactory criterion, though it is a help, and should always be regarded. A consideration of the ratio of the ethereal to the total or preformed sulphates is an important aid to the interpretation of the results.

The value of taking into account this ratio was first emphasized by Van der Velden, and its utility has been appreciated by many subsequent writers. The use of the ratio is based upon the fact that in normal persons the average ratio of the ethereal to the preformed sulphates is not higher than 1 to 10 or perhaps 1 to 9, and is often as low, even on a mixed diet, as 1 to 12 or 1 to 15. Indeed, in our own experience, persons in the best of health have more frequently a ratio in the neighborhood of 1 to 12 than 1 to 10. The sulphates have nothing whatever to do with putrefaction in the intestine, but are derived in the metabolism of proteid food from the sulphur which this contains, and it thus happens that the total sulphates bear a relation to the amount of proteid absorbed, which in turn is related to the amount of proteid food ingested. When, therefore, we say that the ratio of the combined to the preformed sulphates is found to be in the neighborhood of 1 to 10, without much reference to the amount of proteid food ingested, this is very much the same as say-

ing that in normal conditions the opportunity for the activity of putrefactive bacteria in the intestine seems to be proportioned to the amount of proteid food taken. The use of the ratio is thus equivalent to taking into consideration the amount of proteid food ingested and absorbed. The total sulphates, as might be expected, are found to bear a fairly constant relation to the amount of urea—the actual ratio being from 1 to 10 to 1 to 14, in most cases not far from 1 to 12. This ratio, as we have found, is often singularly constant in a given individual. We might, therefore, make our comparison between the ethereal sulphates and the urea; but this is less convenient in practice than to make the comparison with the preformed sulphates. If, however, the sulphates become disproportioned to the urea, as they occasionally do, we cannot rely upon the ratio, but should take into consideration what the ratio would have been if the sulphates had been normal. This rather roundabout course is not thoroughly satisfactory.

It should be mentioned that some writers (F. Muller,[1] Kast and Baas,[2] Salkowski,[3] and von Noorden[4]) object to the use of the ratio on the ground that if we compare the ethereal sulphates which arise exclusively from intestinal putrefaction with the total or preformed sulphates which are derived from the metabolism of proteids and have nothing to do with intestinal putrefaction, we are comparing incommensurable things. While it is perfectly true that the two kinds of sulphates have a different meaning, the considerations above referred to render this comparison reasonable and practically useful, and this view is shared by many actual workers upon the subject, including Bier-

[1] Ueber Icterus. *Zeit. f. klin. Med.*, 1887–XII–63.

[2] *Loc. cit.*, S. 55.

[3] Ueber den Einfluss der Phenylessigsäure auf den Eiweisszerfall. *Zeit. f. physiol. Chem.*, 1889–XII–223.

[4] Ausnutzung der Nahrung bei Magenkranken. *Zeit. f. klin. Med.*, 1890–XVII 529.

nacki,[1] Rovighi,[2] G. Hoppe-Seyler,[3] and E. Baumann.[4] But though we advocate strongly the use of this ratio as a criterion of the output of the ethereal sulphates, we recognize fully the fact that it should not be employed blindly, and that even when employed with every precaution there are occasional cases in which it is difficult to reach a satisfactory conclusion as to whether the ethereal sulphates are present in excess or not.

In the preceding, we have considered some of the fundamental facts relating to the aromatic sulphates as a group. We may now review to advantage certain facts relating to particular members of this group, especially indoxyl-potassium sulphate and phenol-potassium sulphate.

Indoxyl-potassium sulphate, or indican, is practically the most important of the ethereal sulphates, partly because of its relation to pathological conditions, and partly because of the readiness with which its presence is ascertained, which has led to much more general observations upon it.

It is derived from an aromatic volatile substance, indol, which is formed in the intestine from putrefaction of proteids. Indol has the formula C_8H_7N. By oxidation, indoxyl, C_8H_7NO, is formed, which is eliminated in the urine as indoxyl sulphate of potassium, $C_8H_6NOSO_3K$. The latter substance, when pure, crystallizes in white glancing tablets and plates. It was called indican in the mistaken belief that it is identical with the indican of plants, and the name has clung to it on account of its convenience. When this indican undergoes oxidation, indigo blue is formed, and gives to the urine a characteristic blue

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[1] Ueber die Darmfäulniss bei Nierenentzündung und Icterus. *Deutsches Arch. f. klin. Med.*, 1891–XLIX–Hft. 1.

[2] Die Aetherschwefelsäuren im Harn und die Darmdesinfection. *Zeit. f. physiol. Chem.*, 1892–XVI 20.

[3] Ueber die Ausscheidung der Aetherschwefelsäuren im Urin bei Krankheiten. *Zeit. f. physiol. Chem.*, 1888, Bd. XII.

[4] Die aromatischen Verbindungen im Harn und die Darmfäulniss. *Zeit. f. physiol. Chem.*, 1886, Bd. X.

color. Indigo blue is *not* indican; its presence is evidence of the pre-existence of indican in the urine. The reaction by which the change into indigo blue occurs is as follows:

$$2\,C_8H_6NKSO_4 + O_2 = C_{16}H_{10}N_2O_2 + 2\,HKSO_4.$$

Indoxyl-sulph. of potassium. Indigo blue. Pot. hydrogen sulph.

If it is difficult to determine what constitutes an excess of the ethereal sulphates in some cases, it is even more difficult to say exactly how much indigo blue a urine may yield and still be normal. We believe that it is safe to make the general statement that the quantity consistent with a high grade of health and well-being is very small.

The occasional appearance of small amounts of indican in the urine of an adult does not necessarily mean any appreciable disturbance of digestion, but the regular occurrence of even moderate amounts is always associated, in our experience, with evident digestive disorder, and must therefore be regarded as abnormal. It is hardly possible to express by figures the smallest quantity of indigo blue in the twenty-four hours' urine which must be regarded as pathological, for it is necessary in every case to take into consideration whether the patient is robust or feeble, and whether the condition of health is less good when indigo blue is present than when it is not—whether or not symptoms can with reason be associated with its presence. But it is probably correct to say that the quantity of indigo blue in the urine which amounts to a mere trace (corresponding to say one to two milligrammes in the twenty-four hours) can not be considered distinctly pathological, while an amount greater than five milligrammes in the twenty-four hours (an amount which gives a weak but distinct reaction with the Jaffé test) must in the great majority of persons be so considered. An even smaller amount of indigo blue in the urine of children under say five years of age is to be considered pathological. In our experience, we have never met with indican in amounts greater than traces without this being asso-

ciated with evident disorder of digestion. It may indeed be questioned whether in children even a trace can be considered strictly normal. These statements are based upon the examination of a large number of urines from carefully-studied cases.

According to Senator,[1] the urine of newly-born children contains no indican. We have made only a few observations on children at this time, but have never found even traces.

Since indican is an ethereal sulphate, the question naturally occurs to one, Is the relation of this substance to the total ethereal sulphates of the urine a fixed or a variable one? It may be said that there is in general a rough correspondence between them, that when indican is present in large amounts the total ethereal sulphates are apt to be excessive, and when indican is absent we are rather more apt than not to find the total sulphates normal. But to these rules there are so many exceptions that one almost hesitates to speak of them as rules. There are many cases where the ethereal sulphates are normal both as regards totals and ratio, where indican is present in pathological amounts and perhaps in greatly-increased amounts. There are likewise cases, and they are even more numerous than those just mentioned, where the total ethereal sulphates are in pronounced excess, but where indican is altogether absent.

Pathological quantities of indican have been found in a great variety of different conditions. Salkowski[2] has observed such an increase in perityphlitis and peritonitis; Brieger,[3] in chronic anæmias and cachexias; Jaffé,[4] in malignant disease of the intestine; Senator,[5] in chronic

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[1] *Zeit. f. physiol. Chem.*, iv, p. 1.

[2] *Ctrlbl. f. d. med. Wissensch.*, 1876, No. 14, p. 818.

[3] *Zeit. f. physiol. Chem.*, Bd. xi, p. 221.

[4] *Arch. f. gesammte Physiologie*, Bd. xxx, p. 485, and *Ctrlbl. f. die med. Wissensch.*, 1872, Nos. 31–32.

[5] *Ctrlbl. f. d. med. Wissensch.*, 1877, Nos. 20–22.

peritonitis and cancer of the stomach; De Vries,[1] in tubercular peritonitis and obstruction of the small and of the large intestine; Henninga,[2] in pernicious anæmia, typhus, cholera, lead colic, phthisis, acute and chronic gastritis, chronic suppuration in any part, progressive muscular atrophy, and Addison's disease; Gutmann,[3] in the exudate of pleurisy—and this list might be much extended. In every case where indigo blue is obtained in pathological amounts, the condition may be referred to the putrefaction of proteid material, usually within the intestine, but sometimes outside it in the tissues. Just what are the special conditions that give rise to the increase of indican rather than the other aromatic sulphates, we can not say. In all probability, the aromatic bodies formed in putrefaction in the intestine vary, among other influences, with the kinds of bacteria which exist in the intestine. According to Lewandowski,[4] certain bacteria produce both indol and phenol, while others produce either indol alone or phenol alone. But the study of this aspect of the subject is only just begun.

Some observations have been made upon the influence of the digestive secretions in the formation of indol. Thus, according to G. Pisenti, indican depends largely upon the activity of the pancreatic juice. This view is based upon experiments on dogs, in which it was found that the amount of indican in the urine grows much less (after first increasing) when the pancreatic juice is experimentally cut off. Pisenti[5] further regards the disappear-

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[1] Ueber Indican im Harn. *Inaug. Dissert.* Kiel, 1874.

[2] *Deutsch. Archiv. f. klin. Med.*, Bd. xxiii, p. 271–287.

[3] Indigobildende Substance in einem Exudate. *Deutsch. med. Wochenschr.*, 1887, No. 51.

[4] *Deutsche med. Wochenschr.*, 1890, No. 51, p. 1186. See also Baumann, *Zeit. f. physiol. Chem.*, who states that indol is formed in large amount by the comma bacillus of cholera.

[5] Ueber den Zusammenhang zwischen der Einwirkung des Pancreassaftes auf die Eiweisskörper und die Indicanmenge im Harn. *Arch. per le scienze med.* 1887, XII, No. 5.

ance of indican in high fever, when the pancreatic juice is no longer secreted, as bearing out his conclusion. But though these observations are not sufficiently numerous to be regarded as conclusive, even upon the question whether the shutting off of the pancreatic juice regularly influences the formation of indol, it seems to be highly probable that the action of the pancreatic juice in producing tyrosine is an important, though not necessary, condition for the production of indican. We know, further, from the very interesting studies of Ernst, that, so long as the bile is present in the intestine, it is possible to have indol formed in large amount, probably because of the fact that the mucus of the bile furnishes material very favorable for certain kinds of putrefactive decomposition. This latter observation is of especial interest in connection with the circumstance, to be emphasized later, that catarrhal conditions of the small intestine are associated with the output of indican in pathological amounts.

According to De Vries[1] and Filati,[2] the small intestine is exclusively concerned with the formation of indol, in distinction to the large intestine, in which, according to Filati, skatol is the chief aromatic body formed. But this generalization, though attractive, is not as yet based upon sufficient evidence to enable us to accept it without hesitation, though it is to be noticed that the researches of Ernst, mentioned above, accord in some measure with this view.

Opposed to it are the direct observations of Macfayen, M. Nencki, and N. Sieber. They, in a case of intestinal fistula, withdrew the contents of the intestine through a fistula at the lower end of the ileum and found an entire absence of putrefactive products and an almost entire absence of putrefactive organisms, the decomposition present, referable to micro-organisms, being limited to carbohydrates. These observations do not, however, exclude

[1] *Loc. cit.*

[2] *Gazzetta chimica italiana*, 1889, Vol. XIII.

the possibility of the putrefaction of proteids in the small intestine in catarrhal or other abnormal conditions.

A substance which is formed in considerable amount in intestinal putrefaction is carbolic acid or phenol (C_6H_5OH). It may be derived in part in the intestine from the breaking up of tyrosin. It is absorbed as phenol, but somewhere in the body (the liver, according to Baumann) is converted into phenol sulphate of potassium.

Little is known at present as to the occurrence of phenol-potassium sulphate, except that it is present in health as one of the regular constituents of the total ethereal sulphates. It has been found by various observers, that it is increased in some conditions of disease. Thus Salkowski found it increased in ileus and peritonitis, and Brieger in anæmia and cachexias, and von Jaksch[1] states that Strassen has found it regularly present in increased amounts in diabetes, although no change in the sulphate ratio was observed. The methods used for determining phenol included also the cresol present. It was formerly held that the amount normally present did not exceed 0.03 gramme in the urine for twenty-four hours, but more recent methods[2] give much higher results—0.07 gramme and 0.105 gramme.

We do not know what are the conditions that determine a pathological increase of phenol rather than indol, and as yet a knowledge of the amount in which it is present has been made of little practical value. Likewise, practically nothing is known regarding the special significance of the presence of skatol, cresol, and catechol.

We have elsewhere mentioned the occurrence of quinol and catechol in the urine of some cases of epilepsy.[3]

We may now pass to the consideration of some of the clinical conditions in which excessive intestinal putrefaction

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[1] *Loc. cit.*

[2] Kassler and Penny. *Zeit. f. physiol. Chem.*, xvii, p. 117.

[3] Herter and Smith. *Loc. cit.*

is met with sufficient frequency to put it beyond the range of an accidental association.

Excessive intestinal putrefaction in intestinal indigestion.—In recent years there has been a disposition on the part of clinicians to recognize the existence of cases of indigestion in which the symptoms point with reason to intestinal rather than gastric disturbance. In some instances, text-books of practical medicine have taken note of this distinction, but their treatment of the subject must be regarded as inadequate and often misleading. The reason for this shortcoming is to be found in the inherent difficulties of the subject.[1] The symptoms to which intestinal indigestion gives rise vary a good deal in different cases, and are often mingled with the symptoms of unquestioned gastric indigestion. In such instances, it may be impossible to say with certainty what shall be referred to the intestinal disorder. In other cases, the symptoms and signs which would usually direct attention to the intestine are so slight as to be readily overlooked, although the constitutional effects resulting from the derangement may be pronounced. But in many cases, where the symptoms, whether pronounced or equivocal, are those which suggest intestinal rather than gastric disturbance, there are present the objective evidences of excessive intestinal putrefaction. Whether or not signs and symptoms referable to the intestine exist in every case where excessive intestinal putrefaction is present, could be decided only by the most extended and painstaking clinical study. Cases perhaps occur in which such excessive putrefaction gives rise to slight or no intestinal symptoms. But however this may be in exceptional instances, we have met with no case where such excess has been considerable in degree in which there

[1] The readiness with which the gastric contents can be studied accounts for the large literature upon disorders of gastric digestion. The very small extent of the literature of intestinal disturbances of digestion plainly reflects the difficulty of investigating this class of disorders.

have not been also present signs and symptoms which might fairly be interpreted as those of intestinal indigestion. Moreover, in numerous cases, the study of the patient has been sufficiently close to establish beyond question a relationship, in individuals, between the occurrence of the evidences of this excessive putrefaction, on the one hand, and a group of varied symptoms, including local and general manifestation, upon the other.

The following histories are presented here in order to illustrate some of the clinical manifestations occurring in cases in which it is reasonable to consider intestinal indigestion a prominent factor, and in which the objective evidences of excessive intestinal putrefaction were positively ascertained.

These cases are instructive, as showing the coincidence of certain signs and symptoms with the excess of aromatic sulphates in the urine. The cases which have been chosen for presentation illustrate to some extent the variety that exists in the manifestation of intestinal indigestion. We are not yet in a position to describe with entire satisfaction the symptomatology of intestinal indigestion, but will attempt a discussion of its leading features after the clinical histories have been recorded and commented upon:

Case I.—L. H., female, aged thirty years; weight, one hundred and twenty pounds; married. Has always been well, barring occasional periods characterized by abdominal pain, distention, intestinal flatulence, and constipation, associated with loss of appetite and general malaise, the entire disturbance lasting one or two days. Patient came under observation Nov. 5th, 1892. Just previous to this, was in good general condition, with good appetite and no apparent disturbance of digestion; there was slight constipation; stools were large, light brown in color, and natural in odor. Mixed diet, with moderate use of sweets, pastries, etc. About November 5th, patient was troubled with obstinate constipation, intestinal flatulence, occasional slight abdominal pain, and a sense of fulness in the abdomen after meals. A small movement was very offen-

sive in odor, and contained considerable mucus. Appetite good, no headache, sleep interrupted with dreams and intervals of wakefulness. An examination of the urine collected during November 5th (twenty-four hours) gave among other results of analysis the following:

Preformed sulphates	1.826 gramme.
Combined sulphates	0.236 "
Ratio	7.7
Indigo blue	Very strong.
Uric acid	0.430 gramme.
Urea	21.093 grammes.
Ratio	49.0

The significant features of this analysis are the high combined sulphates and the great excess of indigo blue, together with the presence of a normal uric-acid ratio.

The patient's condition improved somewhat after the exacerbation above recorded; the abdominal pain, flatulence, and constipation wore away and sleep became normal. There is no record of the putrefactive products in the urine at this time. On December 7th, the patient again felt poorly, and had some intestinal flatulence and was without appetite. Analysis of urine for the day:

Preformed sulphates	1.433 gramme.
Combined sulphates	0.149 "
Ratio	9.6
Indigo blue	Strong.

At this time, a restricted diet was instituted, consisting chiefly of meat, milk, eggs, with limited vegetable nitrogenous food. Five grains of sodium salicylate were given after each meal. From this time on, doubtless in consequence of the restriction in diet, the ratio of the sulphates fell and remained for some time within the normal. There was, however, little actual improvement, the patient continuing to be troubled more or less with flatulence, slight headache, and general debility. It is noticeable that during this period indigo blue was obtained from the urine in large amount. The figures for this period are as follows:

DATE.	Preformed sulphate, grammes.	Combined sulphate, grammes.	Ratio.	Indigo blue.	Urea.
Dec. 19th, 1892.....	1.454	0.106	13.7	Very strong.	16.125
Dec. 30th.....	1.884	0.180	10.4	" "	23.406
Jan. 11th, 1893.....	1.611	0.109	14.8	" "	18.220
Feb. 2d.....	.....	.....	.....	Very, very strong.	
Feb. 3d.....	.....	.....	.....	Very, very strong.	
Feb 5th.....	2.002	0.151	13.2	Very strong.	25.948
Feb. 6th–8th	4.631	0.275	16.8	" "	47.458
Feb. 10th.....	2.126	0.131	16.2	Medium.	22.622
Feb. 11th.....	2.982	0.180	16.6	Strong.	29.615

On February 12th, the patient's condition was worse than it had been since November 5th, 1892. There was a return of the symptoms present at that time: slight abdominal pain and distention, flatulence, loss of appetite, depression of spirits, and debility. The analysis for this day gave the following results:

Preformed sulphates	1.410 gramme.
Combined sulphates.....	0.181 "
Ratio	7.5
Indigo blue.....	Medium strong.

The urea for this day was rather lower than on previous days, as might be expected, the total being 19, as against 16+, 23+, 18+, 25+, 43+, 22+, and 29+ for previous days. The above results are interesting, chiefly because of the contrast in this ratio of sulphates with the ratio of previous days (the ratio [7.5] being the first above the average normal [10] observed since December 7, 1892), and because of the coincidence of this high ratio with a distinct exacerbation of the symptoms.

On February 23d, the patient's condition was distinctly better than usual, and the analysis for this day showed a marked contrast with that of the day on which it was last recorded. The figures are as follows:

Preformed sulphates.....	2.119 grammes.
Combined sulphates.....	0.158 gramme.
Ratio	13.4
Indigo blue.....	Traces only.
Urea.....	23.0+ grammes.

It should be noticed that for the first time since this case was under observation, the indigo blue amounted to traces only. The next day, February 24th, the condition is described as average. There were still only traces of indigo blue, but the ratio of sulphates was higher (11.3) than it should be on a restricted diet.

On April 6th and on May 9th, the condition being on these days respectively fair and better than usual, the following figures were obtained:

DATE.	Preformed sulphate, grammes.	Combined sulphate, gramme.	Ratio.	Indigo blue.	Urea, grammes.
April 6th.....	1.099	0.68	15.1	Negative.	12.395
May 9th.....	2.065	0.115	17.9	Traces.	23.069

CASE II.—R. M., aged forty-three years; weight, a hundred and forty-five pounds. Mother died of carcinoma of mamma. Brother died of diabetes. Delicate health early in life; pulmonary hæmorrhages at twenty. Gradual improvement in health since then. During past year has been neurasthenic, being troubled especially with sleeplessness, frontal headache, undue emotional excitability, and moderate loss of weight and strength. Late in August, 1892, had severe headache and nausea after overexertion and an indiscretion in diet, consisting of the free use of buckwheat cakes and trout. The pain in this attack was located on the left side of the head, and was very intense at times and lasted several days. There was much prostration at this time, and the temperature ranged between 100° and 101°. When the acute symptoms wore away, it was noticed that the left eyelid drooped considerably, that the left pupil was much contracted, and that the left eyeball was more prominent than usual. Examination showed that sensation on the face was unimpaired, but that the left side of the face was cooler to the touch than the right and paler in color, and that the left side of the face failed to perspire even when perspiration was free on the right side. The fundus was normal, the left supraorbital nerve was extremely tender to pressure, and over the left temple was a small spot of great sensitiveness to the touch.

Before the onset of the present trouble, the patient had suffered from time to time with left supraorbital neuralgia. This now recurred with much greater severity and frequency than formerly.

The patient remained in very poor health for many months. The neurasthenic symptoms above mentioned persisted, and there was almost continual discomfort from the accumulation of gas in the intestine, the bowels being often constipated and sometimes loose. The fæces were light in color, and often contained a large amount of mucus.

An examination of the urine, made September 26th, showed the presence of excessive intestinal putrefaction. During the five months following, most of the examinations made showed some excess of the putrefactive products, especially an excess of indican. The quantity of indican was certainly greatest at times when the patient was most depressed and was suffering from intestinal flatulence and constipation. After the first, the ratio of the sulphates, as shown in the appended table, returned to within the limits of the normal, but this was unquestionably owing to the exclusion from the diet of the vegetable nitrogenous foods.

The results obtained in this case are as follows:

DATE.	Preformed sulphate, grammes.	Combined sulphate, gramme.	Ratio.	Indigo blue.	Urea, grammes.	
Sept. 26, 1892	2.929	0.313	9.3	Medium.	40.105	
Oct. 10th....	.....	.....	.....	Traces.	.....	On greatly restricted diet.
Oct. 17th....	2.825	0.307	9.2	Rather strong.	34.497	
Nov. 2d.....	2.741	0.259	10.5	Medium strong.	34.904	
Nov. 14th..	2.696	0.234	11.5	Weak.	32.498	
Nov. 28th..	3.168	0.225	13.9	Medium.	26.170	
Dec. 5th....	3.443	0.273	12.6	Weak medium.	38.109	
Dec. 19th...	3.050	0.200	15.2	Traces only.	34.566	Improvement on free mixed diet.
Jan. 12, 1893	3.525	0.188	18.7	Medium.	28.49	
May 8th....	2.620	0.279	9.4	Negative.	33.939	
Nov. 12th...	2.752	0.261	10.5	Medium.	33.573	
Jan. 7, 1894.	.....	.....	.....	Negative.		
May 6th	.....	.....	.....	"	35.350	

With the gradual improvement which set in, the urine returned to normal, as regarded the amount of putrefactive products contained in it, and indigo blue was no longer found.

On December 4th, examination of the urine passed just after an attack of headache, gave:—

Preformed sulphates	2.465 grammes.
Combined sulphates	0.210 gramme.
Ratio	11.7
Indican	Strong.
Urea	29.878 grammes.

CASE III.—C. L. D., female, aged thirty years; weight, about a hundred and twenty-five pounds. Well nourished, slightly anæmic. Has been in poor health for several years. At the present time, complains of general debility, lassitude, shortness of breath during exertion, and frequent, slight, frontal headache. Though not disposed to emotional disturbance, the patient is rather inclined at present to mental depression. Bowels are regular, and movements are normal in color and consistence. There is occasional moderate intestinal flatulence. Appetite is fair. There is at times a sense of emptiness and gnawing referred to the upper part of the abdomen. An examination of the urine passed June 4th, 1892, gave the following results:

Urea	19.609 grammes.
Uric acid	0.418 gramme.
Preformed sulphates	1.322 "
Combined sulphates	0.237 "
Ratio	5.5

The patient was put upon a diet consisting chiefly of meat, and from which sweets, starches, and peas and beans were excluded with especial care. Iron and nux vomica were prescribed, and more exercise was advised. In the course of three weeks, there was distinct improvement as regards all the symptoms mentioned above, and the improvement noted at this time was maintained for several months. Unfortunately, no examination was made of the urine during the period of improvement, and the record of the case is therefore defective.

CASE IV.—C. G., male, aged sixty years; weight, a hundred and seventy-four pounds. Has been in poor health ever since his thirtieth year, when he had what were thought to be pulmonary hæmorrhages. Ever since that time he has been neurasthenic, and at times has been disposed to be hypochondriacal. The symptoms complained of at present (April, 1892) have existed in greater or less degree than now for many years. These symptoms are debility, insomnia, pressure sensations in the back of the head, headache, depression of spirits at times very great, loss of memory, "twitching sensations" and fibrillation (especial mornings), poor and capricious appetite and constantly coated tongue, intestinal flatulence with great distention of the abdomen, abdominal pain, alternating diarrhœa and constipation, occasional cardiac palpitation and tachycardia, and frequent passage of gravel in considerable quantities. The progress of the patient's troubles is by no means even; there are days of comparative well-being, followed by days of depression and exacerbation of all the symptoms. It is a noticeable fact that periods of mental depression and debility, insomnia, and impaired memory, correspond to the time at which the intestinal symptoms, flatulence, abdominal distention, etc., are most pronounced.

Examinations of the urine were made from time to time. The results are recorded below. About the time of the first examination, the patient was placed under treatment consisting chiefly of the careful regulation of the diet (which up to this time had been free), and the use of tonics. Vegetable nitrogenous food was excluded, so far as possible, from the diet. Rest was also insisted upon. Some improvement occurred immediately, and in the course of a year the improvement was considerable. This improvement consisted in an increase in the periods of well-being, and a decrease in the periods of depression and intestinal disturbance, so that the average condition was better, notwithstanding the recurrence of most of the symptoms.

DATE.	Preformed sulphate, grammes.	Combined, sulphate grammes.	Ratio.	Indigo blue.	Urea, grammes.	
Mar. 21, 1892	2.061	0.295	6.9	Strong.	29.893	
May 2d.....	2.628	0.236	11.1	"	31.637	
May 23d....	1.826	0.246	7.4	"	24.53	
June 6th....	2.380	0.231	10.3	"	30.425	
June 20th...	2.507	0.257	8.7	"	32.803	
June 26th...	2.101	0.195	10.7	"	29.338	
June 30th...	2.283	0.221	10.3	"	27.172	
July 4th	2.463	0.231	10.6	"	29.364	
Oct. 17th ...	1.623	0.146	9.9	"	23.338	
Oct. 23d....	1.440	0.222	6.4	"	19.298	
Nov. 13th...	2.052	0.214	9.5	Very strong.	29.265	
Nov. 21st....	2.039	0.192	10.6	Very strong.		
Jan. 9, 1893.	1.713	0.192	8.9	Medium strong.	32.626	
May 7th	1.327	0.221	6.0	Strong.	19.523	
May 21st....	1.422	0 154	9 2	"	21.997	Condition poor.
Mar. 19, 1894	1.455	0.155	9.4	"	23.265	Better.

CASE V.—A. L., male, aged sixty years; weight, two hundred and twenty-seven pounds. General health has always been excellent. Is a hard worker, and an abstemious man in every respect. Takes claret with meals and whiskey in moderation. For three years past has been troubled each spring with a general urticaria, which lasted many weeks. Thinks he is run down at these times. Has a "tendency" to rheumatism; is apt to be troubled with painful and rigid muscles, especially those of the neck. Has found of late that he tires very readily, and that sleep does not refresh him as formerly; he gets up tired. Has also been troubled with mental dullness, defective memory, and irritability. The abdomen is most of the time considerably distended by gas, a great deal being passed *per rectum*. At times there is abdominal pain. There is frequent epigastric discomfort, and sometimes tenderness on pressure below the sternum. Some days there is almost constant slight nausea. The stools are usually clay-colored. Examination of the urine gave the following results:

Date.	Preformed sulphate, grammes.	Combined sulphate, gramme.	Ratio.	Indigo blue.	Urea, grammes.
Feb. 9th.....	3.195	0.244	13.1	Strong.	28.553
Feb. 10th.....	2.417	0.256	9.4	Very strong.	26.111
Feb. 14th.....	3.052	0.240	12.7	" "	24.859

On a diet from which sugar and starch and vegetable nitrogenous food was largely excluded, the symptoms above enumerated disappeared very rapidly. At the end of six weeks, there was improvement in the following respects: Sleep was more refreshing, the head felt clearer, memory was better, and fatigue came on less readily. There was also entire disappearance of nausea, and a great diminution in the abdominal distention and flatulence. The rheumatic pains in the neck ceased after a few days, and have not recurred.

An examination of the urine made at this time gave the following results:

Date.	Preformed sulphate, grammes.	Combined sulphate, gramme.	Ratio.	Indigo blue.	Urea, grammes.
April 19th.....	5.531	0.302	18.3	Medium.	44 887
April 20th.....	4.479	0.266	16.8	Strong.	37.715

Shortly after this, the patient indulged in a large dinner, without any restraint, using champagne freely and taking lobster soup, sweets, etc. The day following, he felt dull mentally, and, for the first time in many weeks, there was a return of nausea. There was also considerable intestinal flatulence. Then, returning to a strict diet, improvement again set in. The urine of June 14th and 15th was studied, the patient being in better condition than usual.

Date.	Preformed sulphate, grammes.	Combined sulphate, gramme.	Ratio.	Indigo blue.	Urea, grammes.
June 14th.....	3.625	0.286	12.7	Medium.	39.570
June 15th.....	3.855	0.204	18.9	Weak medium.	25.575

CASE VI.—J. L., female, aged twenty years; weight, about a hundred and forty pounds. General health good, but has suffered since her twelfth year from indigestion. At the present time, suffers from pain in the epigastrium soon after eating, and very frequently there is abdominal pain, associated with great intestinal flatulence and distention of the abdomen by gas. There is very rarely gastric flatulence. While these abdominal symptoms are present, the patient feels weak and somewhat depressed. The sensation of emptiness and exhaustion is relieved temporarily by food. Frequently there is nausea all day long; very rarely there is vomiting. Bowels are regular and fæces are normal in appearance. The patient is a large eater, and takes whatever appeals to her appetite, without much reference to her condition.

Examinations gave the following results:

DATE.	Preformed sulphate, grammes.	Combined sulphate, gramme.	Ratio.	Indigo blue.	Urea, grammes.	
March 16th..	3.971	0.349	11.1	Strong.	51.028	Condition bad; depression and flatulence.
April 26th...	2.169	0.209	10.4	"	29.580	Much depressed and weak; flatulence.
April 27th...	2.826	0.220	12.8	Weak.	43.5	Feeling better; less flatulence and debility.

CASE VII.—H. A. C., male, aged twenty-eight years; weight, a hundred and fifty pounds. Has always been in good health, barring an occasional severe migraine. In October, 1892, had an attack lasting nearly a week, during which there was almost continual abdominal pain, flatulence, diarrhœa, moderate fever and prostration. The stools were not watery, but consisted of large, almost white, cheesy movements, without the characteristic fæcal odor, and resembling the peculiar stools not rarely seen in children with intestinal disturbance. This attack was brought on by eating oysters that were partly decomposed. Ever since this attack, there has been a tendency to the occurrence of slight intestinal disturbance, especially after

the use of lobster and oysters and clams, even when apparently fresh, and after the use of peas and beans. These periods of intestinal disturbance last a day or less, and are sometimes associated with or followed by diarrhœa. They have these symptoms in common: slight or considerable abdominal pain, intestinal flatulence and borborygmi, general malaise, and usually slight mental depression. The urine in this case ordinarily contained no indican, but during the periods above mentioned was regularly found to give a medium strong reaction. The correspondence in this case between the presence of indican and this group of symptoms was strikingly close. For example, on April 30th, 1893, an indiscretion in diet was followed in a few hours later by slight abdominal pain, flatulence, borborygmi, frequent micturition, and depression of spirits. The urine passed (two micturitions) during the remainder of the day gave a medium strong indican reaction. The following day, the symptoms had ceased, and no more indican was found during the following week, although the urine was tested several times each day. On May 9th, the eating of lobster in the middle of the day was followed in a few hours by abdominal pain, flatulence, borborygmi, and diarrhœa. The first urine passed after the beginning of this attack contained indican. After about three hours, the symptoms ceased almost as abruptly as they had begun; the urine passed next gave no indigo-blue reaction. The following morning, there was a recurrence of the above symptoms, without the diarrhœa, lasting several hours, and the urine passed in two successive micturitions following the commencement of this attack gave a medium strong indigo-blue reaction. The indican disappeared from the urine with the disappearance of the symptoms.

The record of this case is defective in that the sulphates were not determined throughout. At times, however, the ratio of the sulphates has been normal when indican was present and intestinal flatulence was complained of.

Case VIII.—H. P., male, aged twenty-two; weight a hundred and forty pounds. Has been in fair health, barring susceptibility to colds in the head and attacks of laryngitis, and frequent acute disorders of digestion. On Febuary 7th,

1893, patient ate a good many fried oysters for breakfast. Next day, he suffered from intestinal pain, borborygmi, flatulence, nausea, and malaise. The pain soon disappeared, but there was more or less intestinal disturbance for many days. Examination made February 10th and 11th showed very strong indican reactions. As the patient improved gradually, the indican and ethereal sulphates decreased in amount, but remained for a considerable time in excess of the normal.

CASE IX.—C. H., girl, aged two years and a half; weight, thirty-two pounds. Has always been in good health and remarkably free from digestive disorders. About January 25th, 1893, without known cause, began to be restless and irritable. There was much loss of appetite, slight constipation, aud slight fever. The stools were rather lighter than normal in color; otherwise, there was no indication of digestive disorder. These symptoms continued with little change until about February 10th. The slight elevation of temperature was almost continual, fever ranging between 99° and 100.5°. During this time, there was loss in weight (about a pound and a half). There was no evidence during this period of the existence of abdominal pain or flatulence or headache.

The urine had been examined repeatedly previous to the period of illness recorded above. At no time was any trace of indigo blue found, and the sulphates were in each instance normal. The results given below represent the conditions found before, during, and after the illness:

DATE.	Preformed sulphate, grammes.	Combined sulphate, gramme.	Ratio.	Urea, grammes.	Indigo blue.	
May 4, 1892.	0.770	0.037	19.8	11.508	None.	Period of good health.
June 21st...	0.997	0.037	25.4	11.119	"	
Oct. 6th.....	1.101	0.035	31.5	12.930	"	
Jan. 30, 1893.	0.654	0.039	16.7	8.249	Medium strong.	Period of illness.
Feb. 3d.....	0.658	0.040	16.4	8.295	Strong.	
Feb. 7th.....	0.640	0.041	15.6	8.392	Medium strong.	
Feb. 11th....	0.692	0.041	16.9	9.084	Medium strong.	Period of improvement.
Feb. 14th....	.....	.....	.....	.....	None.	

Several attacks similar to that recorded have occurred since, and in every instance indigo blue has been present at the time of the fever and digestive derangement, and for some days afterward in diminishing amounts. It is noticeable that the period during which indigo blue persisted in the urine after the subsidence of the acute symptoms of the attack, has been increasing. At present (March, 1893) there is probably a trifle of indigo blue in the urine most of the time. The following results have since been obtained:

Date.	Preformed sulphate, gramme.	Combined sulphate, gramme.	Ratio.	Urea, grammes.	Indigo blue.	
April 14, 1893	0.625	0.038	16.4	7.975	Negative.	
May 10th...	0.801	0.026	30.8	8.523	Traces.	
May 11th...	0.454	0.020	22.7	5.748	Medium.	
May 21st....	0.436	0.018	24.2	4.781	Strong.	Feverish, irritable, appetite poor.
May 28th....	.....	.....	.....	.....	Traces only.	
June 18th...	0.443	0.026	17.0	5.161	Weak.	

Repeated examinations during the year following this time showed considerable variation in the amounts of indigo blue obtained in the reaction, the presence of large amounts corresponding pretty regularly with the appearance of the usual symptoms in the patient. The last examinations are:

Date.	Preformed sulphate, grammes.	Combined sulphate, gramme.	Ratio.	Urea, grammes.	Indigo blue.	
Oct. 3, 1894..	1.361	0.081	16.8	16.727	Strong.	Somewhat irritable.
Nov. 5th....	.....	.....	.....	12.222	Weak.	Condition good.

Case X.—A. K., aged fifteen months. On admission to the Babies' Hospital in December, 1892, weight, seventeen pounds, eleven ounces; tolerably nourished; milk

diet. Soon after admission, the symptoms were as follows: Vomiting, poor appetite, light green or white stools, feeble pulse, somnolence, prostration, and slight irregular elevation in temperature (under 100°).

On December 24th, a little more than a week after admission, the stools were fluid, green, offensive in color, and contained mucus, small greenish masses, and something having the appearance of membrane. The child was in a stuporous state at this time, and it was noticed that there was considerable rigidity of the lower extremities. This rigidity was intermittent and excited by movement or stimulation of the skin.

The intestinal symptoms continued without important changes. Early in February, 1893, there was a considerable increase in the rigidity, which now involved the arms, legs, and trunk, and became continuous. There was great exaggeration of the knee-jerks and typical and violent ankle-clonus. The child cried as if in pain, when touched. No evidence of organic brain or spinal-cord disease. There was a sharp rise in temperature about this time. Loss of weight between admission and February 1st, 1893, nearly four pounds. By March 1st, the general condition had improved considerably: increase in weight; appetite better; stools less often green, frequently normal; temperature normal; less rigidity, and less evidence of pain to touch.

During March, the weight increased rapidly, rigidity became slight, and frequently no clonus could be obtained. By May 1st, rigidity occurred only at times, and, though the knee-jerks were active, ankle-clonus could rarely be brought out, and then only feebly. At no time was there paralysis in the lower extremities or loss of sensibility. The electrical reactions were not taken.

During the period of spasm and pronounced intestinal disturbance, the urine contained very large amounts of indican and high ethereal sulphates. As improvement occurred, these evidences of excessive intestinal putrefaction grew less and finally disappeared entirely.

The results obtained in this case are as follows:

Date.	Preformed sulphate, gramme.	Combined sulphate, gramme.	Ratio.	Urea, grammes.	Indigo blue.	
Feb. 9, 1893.	0.182	0.027	6.7	2.649	Very strong.	
Feb 28th....	0.459	0.028	16.4	.....	Very strong.	
April 18th...	0.2405	0.0365	6.6	3.505	Negative.	Improving.
July 1st.....	.....	.....	.....	.....	"	
Dec. 6th....	0.724	0.059	12.3	8.612	Strong.	
Dec. 18th...	.....	.....	.....	.....	"	
Jan. 18, 1894	.....	.....	.....	.....	"	
Feb. 2d.....	0.954	0.068	14.0	12.985	Very strong.	
Feb. 8th	0 694	0.044	15.7	9.242	None.	Irrigation during these days.
Feb. 11th ...	0.234	0.013	18.0	2.702	Weak.	
Feb. 17th ...	.....	.....	.....	.....	None.	

At one time, the symptoms in this case strongly suggested pressure on some part (cervical) of the spinal cord. It is now a question whether the case may be regarded as one of tetany, notwithstanding the absence of the characteristic contractions.

Case XI.—L. R., aged fifty years; weight, two hundred and twenty-two pounds; well-nourished man. For about a year, he has had occasional periods of nocturnal dyspnœa. These have recently become frequent and very distressing, accompanied with cyanosis, præcordial fear, etc. Examination shows presence of mitral regurgitation and moderate hypertrophy. Symptoms at present are those of broken compensation; dyspnœa, cough, intermittent pulse, slight œdema of feet, and scanty urine. The onset of dyspnœa seems to be occasioned at times by great accumulation of gas in the intestines. For a year past, intestinal flatulence, with borborygmi and distention of the abdomen, has been an annoying and almost constant symptom. No diarrhœa or constipation. No depression of spirits, but a good deal of irritability, frequent headache, and considerable loss of physical endurance. Examination of the urine showed a medium strong indigo-blue reaction, normal sulphates, very excessive uric acid, and considerable oxalate of calcium.

CASE XII.—Mrs. G. W. W., aged thirty-five years; weight, one hundred and fifty pounds; good health until four years ago; then began to have gastric and intestinal flatulence, pronounced and persistent paroxysms of pain in left umbilical region, periods of marked depression of several days' duration, irritability, and loss of strength. These symptoms have continued until the present time. The following are figures obtained while the patient was on a mixed diet and before treatment was attended by improvement. On a diet consisting chiefly of milk and meat, and under the influence of massage and rest, the patient improved very greatly in the course of two months, but the later results on the urine are unavailable because salol was administered. The patient was not constipated:

DATE.	Preformed sulphate, grammes.	Combined sulphate, gramme.	Ratio.	Indigo blue.	Urea, grammes.
Oct. 13, 1894.....	1.386	0.199	6.9	Medium.	17.912
Nov. 11th.....	1.027	0.267	3.8	"	17.140

CASE XIII.—G. S. S., aged fifty years; weight, one hundred and seventy-eight pounds. Suffers from rheumatic pains about hips and sacrum, especially when standing; the pains are dull and fugitive. Is much troubled with insomnia; has bad dreams, and awakens unrefreshed. Has occasional flatulence and abdominal pain. Is irritable and somewhat subject to depression. Is not constipated.

DATE.	Preformed sulphate, grammes.	Combined sulphate, gramme.	Ratio.	Indigo blue.	Urea, grammes.
June 7, 1893.....	1.406	0.167	8.4	Strong.	18.791
June 8th.....	1.462	0.173	8.4	"	19.988

CASE XIV.—C., aged about forty years. Three days ago began to have severe abdominal pain in epigastrium and in right and left hypochondriac regions, with marked local tenderness. Since then has had nausea almost constantly, and has been passing colorless stools. These symptoms entirely disappeared in the course of a week on

a diet of peptonized milk. For many months the patient has been on a diet consisting mainly of cereals.

Date, Nov. 22, 1894.

Preformed sulphate	1.793 gramme.
Combined sulphate	0.363 "
Ratio	4.9
Indigo blue	Negative.
Urea	27.912 grammes.

The histories presented above are all of them the records of cases in which positive evidence was obtained of the existence of excessive intestinal putrefaction by a study of the urine. Further, in most of these cases the chief symptoms were those of intestinal disturbance such as may reasonably be grouped under the name intestinal indigestion. The contemplation of these facts leads one to inquire into the nature of the relation between the putrefactive products observed and the intestinal symptoms, with a view to finding out which of the symptoms of disordered digestion, if any, are regularly associated with the occurrence of the putrefactive products in question.

The symptoms ordinarily referred to disordered intestinal digestion may conveniently be grouped under two heads: the local symptoms, due to the direct effect upon the bowel of the disturbance of its function, and the remote symptoms, due to the absorption of toxic substances from the intestine, or to the reflex influence of irritation of the intestinal mucous membrane.

Of the local symptoms of intestinal indigestion, the most characteristic and the most important for diagnostic purposes is unquestionably intestinal flatulence. By intestinal flatulence is meant the accumulation of gas in the small or large intestine. This gas is usually passed *per anum*, but may be in part eructated by the stomach. It is necessary, of course, to distinguish intestinal from gastric flatulence, and to bear in mind that gas, formed in consequence of gastric indigestion solely, may be passed on to the intestine. The degree of flatulence varies greatly. In

many cases there are no pronounced evidences of the presence of gas except the frequent passage of wind by the anus; there is no appreciable abdominal distention. In other cases, there is marked abdominal distention, and not rarely slight pain, or, at least, a sense of discomfort, in the abdomen due to excessive peristalsis. The distention of the abdomen with gas may be a most distressing symptom. In Case IV, frequent great abdominal distention was, perhaps, the most annoying symptom, and its presence seemed, in several instances, to occasion the onset of attacks of tachycardia, lasting several hours, and disappearing with the relief of the distention. In Case XI also, great abdominal distention was of frequent occurrence, and at times appeared to determine the onset of the nocturnal dyspnœa due to the mitral insufficiency.

The association of intestinal flatulence with abdominal pain and with excessive peristalsis (symptoms often following palpable errors of diet) at one time made it appear probable that this symptom was to be considered an evidence of disordered intestinal digestion. But observations made upon the urine of persons having intestinal flatulence failed to furnish good evidence that such flatulence is usually associated with excessive putrefactive processes in the intestine. In some cases, however, where a sufficiently careful record has been kept to enable a judgment to be formed, there has been a close correspondence between the existence of flatulence and the presence of indican in the urine. This was so in Cases I, II, IV, and VII, and in some cases not included in the preceding histories. In Case VII, in which there were frequently repeated slight attacks of intestinal indigestion, the urine was examined several times daily for many weeks, and the correspondence between the occurrence of flatulence and the appearance of indican in the urine was strikingly close. In every instance, the urine passed after the beginning of flatulence gave a medium or weak indican reaction, which disappeared

as soon as the symptoms wore away. In no instance did the urine passed in the intervals between the intestinal disturbance contain any indican. In Case I, the diminution of indican coincided with marked diminution in the flatulence and other intestinal symptoms. In Case II, the indican was greatest in amount at the time when intestinal flatulence was an annoying symptom. Since the disappearance of indican from the urine, there has been entire relief from flatulence, although certain other intestinal symptoms, as light-colored and mucous stools and constipation, have recurred from time to time. In Case IV, there has been a decided reduction in the amount of indican in the urine, and a reduction in the degree and frequency of intestinal flatulence, together with distinct improvement in other respects. But though the indican has decreased, it has not disappeared, and has been found in considerable amount when there was almost entire freedom from flatulence. In Case V, again, there was a pronounced reduction in the degree and frequency of intestinal flatulence, with only a slight reduction, if any, in the intensity of the indigo-blue reaction. In Case XI, on the other hand, there appears to be a close relation between the degree of flatulence and the presence of indican. In Cases IX and X (children), it was not possible to study carefully the intestinal condition as regards flatulence. In Case XI, there was a great deal of flatulence habitually, and a single examination showed a strong indigo-blue reaction.

So far, then, all that has been established is the fact, that in a number of cases the occurrence of intestinal flatulence was associated with the presence in the urine of indican; that in some cases this correspondence has been remarkably close, but that there is no necessary relation between the two conditions. Nothing has been said as to a possible relation of flatulence to the total ethereal sulphates of the urine.

A survey of the preceding cases in connection with the

figures respecting the total ethereal sulphates shows very clearly that there is no necessary relation between flatulence and an abnormally high ratio of the combined to the preformed sulphates.

In Cases I, II, VII, IX, and XI, the ratio of sulphates was habitually within the normal, notwithstanding the occurrence of indican and of flatulence. In Cases IV, V, VI, VIII, and X, the ethereal sulphates were excessive (judging from the ratio) and indican was present. In Case III, the ratio of the sulphates was high, but there was neither indican nor flatulence.

Thus we have to recognize the existence of two classes of cases of intestinal disorder: 1, cases in which there is flatulence and usually only a moderate amount of indigo blue, and in which the indigo blue disappears from the urine when the flatulence is removed by means of a change in diet; and, 2, cases with or without continuous flatulence, in which there is a large amount of indigo blue in the urine, but in which there is little or no change in the amount of indigo blue in the urine when the flatulence is controlled. A large quantity of indican is therefore a permanent feature in these cases. How do these cases differ from those of the first class? It is not unlikely that the explanation of the difference is as follows: In the first set of cases, we may assume that the disorder is primarily one of putrefaction of nitrogenous food, due to an indiscretion in diet or to the deficient or perverted action of the bile or pancreatic juice, or to both these causes together. Indol is formed by the decomposition of the food during duodenal digestion, and, with this, various gases arise, whose accumulation causes flatulence. That the food is really at fault in these cases, is shown by the circumstance that a modification of the diet by the use of nitrogenous food of the kind readily digested, and which is thus least apt to undergo putrefactive changes, is followed by the disappearance of the flatulence and indigo blue.

In the second class of cases, it is probable that the food is not at fault primarily, since the modification of the diet has relatively little effect upon the indican. From the constancy of the indican, it seems probable that there is present a persistent condition of the intestine concerned with the production of indol. We have already noted the fact that indican is formed from the mucus of the bile, and it occurs to one that in these cases the putrefaction may depend on the presence and stagnation of an excessive amount of intestinal mucus from the bile or other source. The persistence of this condition indeed leads to a suspicion that somewhere in the duodenum the mucous membrane is in a state of catarrhal inflammation, to which the excessive formation of mucus might reasonably be referred. The absence of flatulence at times in these cases would be explained by the fact that the food does not necessarily share in the putrefactive process. We have, of course, no proof that this view of the case is the correct one, but it seems to harmonize with what is known about the causes of intestinal putrefaction. It is interesting to note in this connection certain of the facts relating to the occurrence of intestinal putrefaction in starvation. These facts are, the occurrence of large quantities of indican in men and animals in starvation, the absence, so far as known, of flatulence, and the accumulation in the intestine, in this state, of a peculiar kind of fæces, consisting largely of broken-down epithelial elements, mucus, etc.

Abnormities in the fæces, as shown by changes in their appearance or in their chemical characters, are of frequent occurrence in the subjects of intestinal indigestion, and were noted in many of the cases recorded above. As might be supposed, no relation has been established between the occurrence of such abnormities and excessive intestinal putrefaction, and it would take far more careful studies than any which have yet been made to determine whether or not such a relation exists. A number of things

seem to indicate that many of the peculiarities in the fæces seen in cases where excessive intestinal putrefaction occurs, are to be considered evidence of intestinal indigestion.

Thus the presence of clay-colored stools is a feature of many cases where the sulphates are excessive and indican is abundant, and there is some reason to ascribe this condition to the defective secretion of pancreatic juice.[1] Again, the presence of fat in excess in the fæces (*i. e.*, much more than twenty per cent. of the solid matter) is frequently met with, and is surely to be regarded as due to defective secretion, either of the pancreatic juice or the bile,[2] or both. Another point which it might pay to study, is the ratio of the fatty acids in the fæces to the neutral fats, since a marked disturbance of the normal ratio (say from eight to forty per cent.) is regarded by some authors[3] as positive evidence that the pancreatic juice is not doing its part in intestinal digestion. It would also be of interest to note carefully the quantity of stercobilin in the fæces, this being very abundant in some persons having an excess of the putrefactive substances in the urine. This is, however, also found in excess in persons in whom no excess of intestinal putrefaction is present. A very hurried passage of partially digested or almost unchanged food along the alimentary canal is apt to occur from time to time in those who are the constant subjects of excessive intestinal putrefaction, but it is doubtful if this is any evidence of defective or altered intestinal secretion. It is, rather, evidence of defective absorption and excessive peristalsis. But it is probably fair to regard its occurrence as one of the signs of intestinal or gastro-intestinal indigestion. There is reason to believe that during such active peristal-

[1] Walker, quoted by Thacher.

[2] See Thacher, *New York Medical Journal*, 1893.

[3] Leo, also Müller. In one case, the neutral fats were present in very small amount, the fat of the fæces consisting chiefly of soaps and fatty acids. Here there was probably an excessive fat-splitting action on the part of the fermentative bacteria, due probably in this case to the reduction in bile. See Biedert.

sis, aromatic putrefactive products are absorbed from the intestine in only very small amount.[1] The occurrence of considerable mucus in the stools is a common occurrence in intestinal indigestion, and doubtless often depends on an enteritis. It does not appear to bear any close relation to the occurrence of intestinal putrefaction.

The influence of constipation on the appearance of putrefactive substances in the urine, has been discussed elsewhere. It is important to note here the frequent occurrence of constipation in those who have the symptoms of intestinal indigestion. It is, however, certain that constipation, using the term in its ordinary sense, can not be solely responsible for the increase of putrefactive products found in the urine, in intestinal indigestion. This is shown by the fact that in some cases these products continue present in excess, and with little or no diminution after the constipation has been relieved. But it should be remembered that there may be in some of these cases an abnormal delay in the passage of fæces through the intestine, although there is a daily movement. Such a condition, if it occurs, would constitute a virtual, if not an apparent, constipation, and might be responsible, in a measure at least and in some cases, for the appearance of high ethereal sulphates. In order to be able to judge regarding this point, we ought to have more information about the rapidity with which food normally makes its way through the intestine.[2]

A minority of the subjects of intestinal indigestion com-

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[1] Some work has been done which indicates that after severe diarrhœa the ethereal sulphates are apt to be greatly reduced in amount. See S. 7. Bartoschewitsch. Zur Frage über das quantitative Verhalten der Schwefelsäure und der Aetherschwefelsäuren im Harn bei Diarrhoen. *Zeitschrift für physiologische Chemie*, 1892–xvii–Hft. 1. Also E. Baumann. Zur Frage der Aetherschwefelsäureausscheidung bei Cholerakranken. *Zeitschrift für physiologische Chemie*, 1893–xvii–Hft. 5.

[2] Some patients have been able to judge of the rapidity of this progress by the appearance in the fæces of spinach taken with the food. It is probable that food normally passes through in somewhat less than twenty-four hours.

plain at times of some form of abdominal pain. Sometimes the pain is spasmodic and migratory, being referred first to one part of the abdomen, then to another—usually to the lower part of the abdomen, and frequently to one or other side. This colic is commonly associated with flatulence, and perhaps with some degree of abdominal distention. There is, of course, no doubt as to the dependence of this pain upon spasm of the muscular coats of the intestine. It is due probably, in most instances, to the irritation produced in the course of putrefactive changes in food during intestinal, probably pancreatic, digestion. The pain may, it is said, have nothing whatever to do with intestinal indigestion, and be of centric origin, so that we can not always regard it as indicating intestinal indigestion. The epigastric pain, which comes on in some of the patients who have excessive intestinal putrefaction, is more difficult to interpret. The pain varies much in intensity in the same and in different persons, and in the time of its onset after a meal. It may be difficult to decide whether, in a given case, such pain is gastric or duodenal. The fact that in some cases it is relieved by washing out the stomach, seems to show that it may be of gastric origin. According to Golding Bird, undue relaxation of the pylorus, with a rapid escape of the stomach contents into the duodenum, is the explanation of the pain in the intestinal cases, but it is doubtful whether this is really the explanation. The length of time at which the pain comes on after eating does not, as pointed out by Allchin,[1] aid in the determination of the origin of the pain, for gastric and duodenal digestion progress, to a considerable extent, simultaneously. In some cases where there is excessive intestinal putrefaction, there is not only severe and continuous epigastric pain, but also great tenderness on pressure over a limited area an inch or two below the sternum, suggesting organic stomach

[1] On Duodenal Indigestion. *Lancet*, 1890.

disease. We do not know how and where this pain and tenderness are caused, and it is not our intention to inquire here into its nature. The same symptoms occur, however, in some persons with enormously excessive uric-acid excretion without excessive intestinal putrefaction, and in other cases, where such putrefaction exists, have cleared up under treatment which did not affect the putrefactive products in the urine. We may conclude, therefore, that these symptoms—epigastric pain and tenderness—probably are in no wise dependent on intestinal putrefaction, even though they may be of intestinal origin in some cases.

Another symptom of frequent occurrence in cases where intestinal putrefaction is excessive and referred to the epigastrium, is a feeling of emptiness, described by some patients as a "gone feeling." In some patients this is a very prominent and distressing symptom. The feeling is usually intermittent, and is apt to be particularly pronounced when the patient is tired, especially late in the afternoon. An empty stomach is a condition favorable to its development, and the taking of food usually relieves the sensation, at least for a time. That an empty stomach is in some cases not essential to its production, is shown by the fact that a considerable quantity of food may give relief for only a few minutes. According to Golding Bird, this symptom also may be due to the too rapid passage of food into the duodenum, for this feeling of faintness, together with flushing of the face, rapid pulse, etc., made its appearance in a case of jejunostomy when from fifteen to twenty ounces of food were introduced directly into the intestine, but did not appear when the quantity mentioned was much reduced. But, whatever may be the origin of this sensation, whether it be gastric or duodenal, it is certainly not directly related to excessive intestinal putrefaction. This is shown by the fact that it often occurs in cases where putrefaction is not excessive, and that in other cases it is relieved without any corresponding lessening of the putrefactive products in the

urine. In these respects, the symptom resembles epigastric pain, with which it is not rarely associated. It is related to epigastric pain in still another way—namely, in its frequent occurrence in persons who are excreting an excessive amount of uric acid, and in the relief of both symptoms when the diet is so altered as to diminish the uric-acid output materially. These facts lead one to suspect that both epigastric pain and the sensation of goneness may be due at times, and in part at least, to imperfect or perverted digestion in the small intestine, notwithstanding the absence of any relation to excessive putrefaction in the intestine. In other words, we must be prepared to recognize the existence of intestinal indigestion without excessive intestinal putrefaction. We do not wish to be understood to mean that epigastric pain and faintness and uric-acid excess are conditions necessarily dependent on intestinal indigestion. It is highly probable that all these conditions may depend sometimes on disturbance limited to, or at least initiated in, the stomach. Stomach indigestion is associated more often than not with intestinal indigestion, and the defective preparation of the food in the stomach for intestinal digestion, through defective secretion, for example, of hydrochloric acid, is thought to be a cause of intestinal indigestion, and of excessive putrefaction.

In this connection, it is to be also remembered that gastric hydrochloric acid normally exercises a retarding influence on bacterial activity in the small intestine, and that hence a diminished secretion of this constituent of the gastric juice constitutes a condition favorable to putrefactive processes.

The digestive process is, however, more largely carried on in the intestine than in the stomach, and there is reason to think that, in most cases where excessive uric-acid output depends on digestive derangement, it is due chiefly to intestinal indigestion.

The only remaining local symptom of intestinal indiges-

tion is nausea. Nausea, like vomiting, is certainly more often a gastric than an intestinal symptom. But cases are occasionally met with in which nausea, lasting perhaps several hours daily, is accompanied by symptoms, such as flatulence and clay-colored stools and abdominal pain, which prove the existence of intestinal indigestion, and unaccompanied by any symptoms that point to gastric indigestion (acidity, heartburn, pyrosis, gastric flatulence). This nausea, which under these circumstances may be a most distressing condition, is quickly relieved by a diet (such as an exclusive meat diet) which relieves the other evidences of intestinal indigestion. Nausea of intestinal origin probably stands in no direct relation to the element of excessive intestinal putrefaction, though it frequently is associated with such excess.

The remote symptoms of intestinal indigestion are exceedingly numerous, and occur in combinations so varied as to baffle adequate description. Some of the following symptoms are usually associated with the local symptoms of intestinal indigestion that have just been referred to: malaise, habitual or occasional; lassitude; aversion to exercise which rapidly fatigues; involuntary muscular twitchings (occasionally observed), especially of the muscles of the back; flushing of the face, particularly after fatigue or after food; cold and moist extremities; irregular and habitual rapid heart action, perhaps attacks of tachycardia; drowsiness during the day; insomnia at night; hypochondriacal periods, or periods of slight mental depression; impaired appetite; loss of weight; periods during which there is very frequent micturition. Most of these symptoms, when continued, are the symptoms of neurasthenia, and there can be no doubt that chronic intestinal indigestion is one of the most frequent and effective causes of the neurasthenic condition. When we come to inquire into the relation of the remote symptoms enumerated above to the state of excessive intestinal putrefaction

so often the important factor in intestinal indigestion, we find the task a difficult one, for we find the facts to be as follows: 1. Many of the remote symptoms enumerated occur in persons in whom there is no indication whatever of intestinal putrefaction in excess, but in whom too much uric acid is being excreted, and who have some of the local signs of intestinal indigestion. 2. There are cases of intestinal indigestion in which the products of intestinal putrefaction are in excess, but in which the remote symptoms enumerated are not pronounced. These facts, in the form just stated, might lead us to think the occurrence of excessive putrefaction, with its remote symptoms, to be accidental, and not expressive of any causal relationship. When, however, the following considerations are borne in mind, we see that there is no ground for entertaining this view, for (1) the remote symptoms which occur where there is intestinal indigestion, without excess, in the putrefactive products are slighter in degree than when such excess becomes associated with it, and (2) remote symptoms are seldom or never entirely absent when excessive putrefaction is present, and are usually very pronounced when such excess is large. But, while we must recognize the fact that the products of excessive intestinal putrefaction are related, in some way, to the occurrence of the remote evidences of intestinal indigestion, it must be owned that we can not distinguish clearly between the symptoms that occur in this association and those that arise where the intestinal indigestion is unassociated with such excess, but with too large a uric-acid output. It may be that a difference exists in the nature of the symptoms in the two sets of cases—indeed, it is likely—but much more extended study is required to bring it out.

It has seemed as though mental depression was especially likely to occur when the ratio of the sulphates is very low (lower than one to five), and it is possible that this is so; but the same symptom has been noted with very high

uric acid, and no abnormity in the sulphates, and occasionally the sulphates run very high without appreciable mental depression.

We are likewise in the dark as to the manner in which the remote symptoms arise in cases where the putrefactive products are present in excess, and in the present state of our knowledge a discussion of the possibilities is futile. It may, however, be pointed out here that there does not seem to be much likelihood that the ethereal sulphates are in themselves responsible to a considerable degree for the remote symptoms.[1] It is more probable that these symptoms depend largely upon the absorption from the intestine of toxic substances formed there *coincidently* with and in consequence of the putrefaction, of whose extent the ethereal sulphates are the index. At the same time, we are in no position to deny the possibility that the various aromatic substances excreted as sulphates are in themselves capable of exerting some toxic effects upon the organism, and it is not unlikely that under some circumstances this influence may even be considerable.

One variety of the occasionally remote symptoms deserves special notice—the ocular symptoms. In Case II, there was developed a paralysis of the left sympathetic nerve, during a period of acute gastro-intestinal indigestion, accompanied with fever and prostration. The development of sympathetic palsy at the time of an acute digestive disorder, traceable to an indiscretion in diet, may have been mere coincidence. It occurs to one, however, that the transient disturbance in function in the sympathetic nerve may have been due to the absorption from the intestine of toxic substances generated within it. In this instance, there is the notable circumstance that the eye affected is upon the side of the recurrent supraorbital neuralgia which has been a feature in the case, and, further,

[1] Note the large excretion of sulphates from the use of salol, etc., without symptoms.

that at the time when the palsy developed, there was a sharp attack of supraorbital neuritis upon this side.

The following case of ophthalmoplegia, in which the paralysis came on during an acute gastro-intestinal disturbance occasioned by the use of cheese, is highly suggestive in this connection. Unfortunately, no studies of the urine were made, and we can not express any opinion as to the existence of excessive intestinal putrefaction at the time of the attack. This case occurred in the practice of Dr. George W. Jacoby, to whom I am indebted for the use of the following history:

M. B., male, aged forty-four years. Well until September 10th. On the evening of that day, he and his wife went to a restaurant and ate Swiss cheese and sausage. It was noticed that the cheese was not good. He ate more than his wife. During the night, his wife complained of nausea, and vomited undigested cheese and sausage; he also complained of nausea, but did not vomit. On the following day he suffered with nausea, loss of appetite, coated tongue, constipation. Toward evening, faintings, vomiting, slow, weak pulse; almost in a state of collapse. He was given stimulants; and castor oil. Copious evil-smelling movements for several days, pains, and great prostration.

September 15th.—Great dizziness and *diplopia*. Says he has seen double, off and on, for several days. Examination: Almost complete external ophthalmoplegia, both sides, left more than right; the rectus superior seems to be less involved than other muscles. Pupils react normally; nothing else; facial, hypoglossal, special senses, reflexes normal; gradual improvement.

October 1st.—Complete restitution.

In a recent study of the ætiology of oculomotorious paralysis, A. Dalichow[1] calls attention to the occurrence of

[1] Aetiologie und ätiologische Diagnostik der Oculomotoriusparalyse. *Zeitschrift für klin. Med.*, 1893-xxii-Hft. 516. Accommodationslähmung und sonstige Störungen der Augen bei Wurstvergiftung. *Archiv. f. Ophth.*, 1880-xxii-236. See also Scheby-Buch. Bericht über 38 Fälle von Accommodationslähmung aus den Kieler Kliniken, darunter befinden sich 5 Fälle solcher

ocular palsy after the use of spoiled meat, especially sausage meat. The extent and distribution of the paralysis appear to vary with the amount of the absorbed poison. When this is slight, the paralysis involves especially the intrinsic ocular muscles, and in the severest cases, some of the external muscles. In connection with the cases here reported, the following notes of the symptoms in a case of sausage poisoning reported by Leber are of interest:

A short time after the ingestion of the sausage meat, the patient began to suffer from nausea and vomiting, great thirst, dryness of the throat, and discomfort in the joints. Next day, there were stoppage of the salivary secretion and dry and white skin. At the same time ocular symptoms appeared—stoppage of lacrymal secretion, ptosis, double vision, mydriasis, and paralysis of accommodation. These ocular symptoms persisted for some time after the disappearance of the general symptoms.

Excessive Intestinal Putrefaction in Epilepsy.—In a preliminary paper, to which reference has already been made, reports were presented as to the condition of intestinal putrefaction in thirty-three epileptics, and it was pointed out that some degree of excess of the products of such putrefaction was found in the urine in the majority of these cases. Since the publication of this paper a number of further observations have been made, partly upon the cases previously reported and partly upon new ones. These observations confirm the accuracy of the conclusions already reached. As might have been predicted, the more extended study of the general theme which forms the subject of the present article has served to emphasize certain facts regarding the earlier work, especially the fact that the excessive intestinal putrefaction is a condition

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durch Wurstvergiftung. V. Graefe, *Archiv f. Ophth.*, 1891-xxvii-265. Groenour, A. Fünf Fälle von Accommodationslähmung bei Fleischvergiftung (Schinken) aus der univ. Klinik f. Augenkr. in Breslau. *Klin. Monatsbl. f. Augenheilkunde*, May 28th, 1890.

of wide distribution, and in no wise distinctive of, or peculiar to, epilepsy. It seems almost unnecessary to point out this fact here, but it appears that the report already mentioned has given some the impression that a claim was made that excessive intestinal putrefaction is distinctive of epilepsy. Although it is owned that the broad distribution of this condition was less fully appreciated formerly than now, it was fully understood that the condition was not in any way peculiar to epileptics,[1] though in them it seems frequently to determine seizures.

The results in the following cases of epilepsy, which have been studied during the past years in regard to the subject under consideration, may be very briefly presented:

A. M., aged twenty-six years; weight, one hundred and sixty pounds; has had several seizures of typical *petit mal* daily for more than ten years. Examination showed that there was no deviation from the normal as regards putrefactive processes in the intestine, although the seizures were recurring with the normal frequency at the time. The uric acid, however, is somewhat in excess. The figures are as follows:

Urea	26.676 grammes.
Uric acid	0.658 gramme.
Preformed sulphates	1.864 grammes.
Combined sulphates	0.173 gramme.
Ratio	10.700 "

This case falls in line with the cases of *petit mal* previously reported in being negative as regards putrefactive excess, and in showing a high uric acid, which is doubtless to be attributed to some disorder of nutrition.

The next case was in the practice of Dr. Frederick Peterson, to whom I am indebted for the history.

L. L., male, aged twenty-four years, has had idiopathic nocturnal *grand mal* seizures every few months for about five years, and *petit mal* seizures several times weekly. On dietetic treatment, with strontium bromide. Examina-

[1] There is no formal statement of this fact in the text, but the occurrence of excessive intestinal putrefaction in Bright's and jaundice is mentioned in one of the foot-notes. *Loc. cit.*

tion showed a high grade of intestinal putrefaction. The figures are as follows:

DATE.	Preformed sulphate, grammes.	Combined sulphate, gramme.	Ratio.	Urea, grammes.	Indican.
Oct. 2, 1892.....	1.502	0.248	6.0	18.397	Strong.
Oct. 8th.....	1.438	0.207	6.9	17.126	"
Oct. 29th.....	1.205	0.336	3.6	17.715	Medium (violet).
Nov. 2d.....	1.753	0.179	9.8	19.668	Medium.

There was slight improvement on hydronaphthol in this case. General condition improved, but no permanent effect on seizures.

In another case of epilepsy, with frequent atypical *grand* and *petit mal* seizures, in a corpulent boy aged twelve years, in which there was a suspicion that the trouble was of traumatic origin, but in which trephining did no good, a study of the urine showed the existence of excessive intestinal putrefaction. On a restricted diet there was a reduction in the putrefactive products, but no diminution in the frequency, or change in the character, of the seizures.

I am indebted to Dr. Starr for the opportunity to study the case. The results were as follows:

DATE.	Preformed sulphate, grammes.	Combined sulphate, gramme.	Ratio.	Urea, grammes.	Indican.
Oct. 11, 1892.....	2.909	0.340	8.5	34.011	Very strong.
Oct. 12th.....	2.914	0.231	12.6	30.163	" "
Oct. 13th.....	2.516	0.227	11.0	27.310	" "
Nov. 10th.....	3.457	0.263	13.1	32.783	Very weak.
Nov. 11th.....	1.301	0.126	10.3	15.122	Negative.
Nov. 23d.....	3.230	0.245	13.2	.....	Very strong.

After treatment by intestinal antiseptics and diet had been given up as useless, considerable improvement occurred with the bromide treatment.

In the following case, which occurred in the practice of Dr. Starr, and is probably one of epilepsy, the results of a single examination, made a few days after the last seizure, were entirely negative, as regards evidences of excessive intestinal putrefaction. The patient, a man thirty years of age, gave a history of having had, in the course of three months, three seizures, characterized by nausea, vomiting, and subsequent loss of consciousness, in which he "thrashed about." These are the figures:

Urea	25.962 grammes.
Uric acid	0.606 gramme.
Preformed sulphates	2.170 grammes.
Combined sulphates	0.130 gramme.
Indigo blue	Negative.

The three following cases occurred in Dr. Northup's service in the Presbyterian hospital. Very careful histories of the cases were furnished me by Dr. D. Van B. Hedges, the house physician.

The first case is that of a little boy, aged four years, who for a year prior to admission had been subject to general epileptiform seizures, which, during the month preceding admission, had recurred as frequently as three or four times daily. Soon after admission, he was put on a carefully regulated diet, from which vegetable nitrogenous food was excluded, and took five grammes of resorcin three times daily. On the fourth day after admission, the convulsions ceased. He remained in the hospital, under observation, for two months, but had no return of the seizures. Unfortunately, no examination was made of the urine until the patient was put upon treatment. It then contained a small amount of indigo blue. It is likely, though by no means certain, that an examination made before treatment was begun, would have given more decided evidences of excessive intestinal putrefaction. The case is recorded here because of the very striking improvement that occurred when treatment was commenced. The most careful examination failed to show any evidence of former organic cerebral disease. There had been no restrictions in the diet of the child before he came under observation.

The next case illustrates the failure of the treatment which, in the preceding patient, seemed so efficacious.

A boy, aged sixteen years, with the habit of masturbation, had been having frequent typical *grand mal* epileptic seizures, two or three a week, for two years before admission to the hospital. The seizures begin regularly with a sense of numbness in the left hand, followed by local spasm, first of the finger, then of the entire left side, and finally become general. For three days after admission had a convulsion at about 6 a. m. On resorcin, five grains three times a day, there were no seizures for ten days, and the patient left the hospital apparently improved. But the seizures soon returned, and he was readmitted. For three weeks following readmission, there was a seizure each morning about six o'clock, notwithstanding an attempt to diminish intestinal putrefaction by the use of large doses of resorcin, careful diet, and the free use of bromides.

In this case apparent improvement followed upon the use of strychnine and antipyrin.

Studies of the urine give the following results:

Date.	Preformed sulphates, gramme.	Combined sulphates, gramme.	Ratio.	Urea, grammes.	Indican.
April 10, 1893.....	0.725	0.316	2.3	12.959	Negative.
April 11th.....	0.548	0.312	1.7	15.899	Medium.

T. M., aged twenty-six years, was admitted in a state of unconsciousness, having been picked up in an epileptiform seizure. After admission, had fifteen general epileptic convulsions in the course of seven or eight hours. Was delirious in the intervals. Temperature rose to 104° F. The following day, the temperature fell to normal, and there were only three seizures. On coming out of the last one, the patient was very noisy. Three days later there were two slight seizures, and, during the ten days following, none.

The interesting feature of this case is, that at the time the patient was having such numerous seizures, the urine

gave an intense indigo-blue reaction, which grew less and disappeared when the seizures ceased. A slight amount of indigo blue was present when the seizures returned, but during the remainder of the patient's stay in the hospital, none could be detected.

We are not able to make any decisive statement regarding the symptoms of intestinal indigestion in epilepsy, in cases where putrefactive products are present in excess, since in most cases the digestive symptoms were not inquired into with care.

In many cases, however, especially those in which indican has been present in excess, there have been symptoms pointing to intestinal indigestion, and there is every reason to suppose that the symptoms of indigestion occur under the same conditions as when epilepsy is not a feature of the clinical history.

Excessive Intestinal Putrefaction in Melancholia.—In July, 1892, Dr. John Macpherson[1] called attention to the results obtained by him in the treatment of some forms of acute insanity by means of intestinal antiseptics. He pointed out the fact that the acid of the gastric juice has important antiseptic properties, and that where this secretion is perverted, as in acute mental disease, we have an indication for the use of intestinal antiseptics.

The mode of procedure recommended is as follows: Selecting a suitable case, the stomach is first washed out; a dose of calomel (two and a half to four grains) is then given at night, followed by a mild cathartic in the morning. This is followed by the use of mild laxatives daily. On the second day, naphthalene, in ten-grain doses, three times daily, is given between meals. The dose is gradually increased until eighty grains have been administered in the twenty-four hours. No harmful effects were noted from these doses. The quantity of nitrogenous food taken

[1] Abstract in one of the December, 1892, numbers of the *New York Medical Journal.*

during the treatment was much decreased. Macpherson treated thirty acute cases of mental disease in this manner. He observed that the bodily weight steadily increased. The tendency of the skin to pigmentation in melancholia was checked, and the skin lost its dry appearance.

The action of the drug in promoting sleep is said to have been pronounced. Macpherson considers naphthalene of especial value in acute melancholia. He states that the naphthalene reduced the sulphates in the urine (a vague statement, as reported), and that the fæces of patients using it were devoid of smell—evidences of the antiseptic power of the drug.

Through the interest of Dr. Atwood and Dr. Dold, of Bloomingdale Asylum, we have had an opportunity of studying the condition of intestinal putrefaction in eight typical cases of acute melancholia. It will be noticed that the most striking thing about the results in these cases is the fact that the ratio of the sulphates is lower than in health, and not merely lower, but very much lower, the ratios being among the lowest ever observed in disease. At first sight, it might be supposed that we had to deal here with an unequivocal example of excessive intestinal putrefaction. On reflection, however, it appears that any conclusion based merely on this low ratio (which we may assume for the moment to be a characteristic of melancholia, though the number of cases is not great) fails to take account of certain unusual features present in these cases, which suggest that this ratio should be differently interpreted. It is noticeable first, that the preformed sulphates are present in very small amount, and that this circumstance gives us a low ratio, notwithstanding an unusually small amount of combined sulphates—an amount which may be less than is usually observed in health. Further, it is evident that the quantity of urea excreted is, in some instances, low. Now, both the low urea and the low preformed sulphates indicate inactivity in nitrogenous

metabolism. This is accounted for, probably, by the small quantity of food taken in these cases. In fact, as is well known, cases of melancholia are often in a condition of partial starvation.

The quantity of food taken in Case I was considerable, but in the other cases was small. We know from recent observations of Müller[1] that in persons who starve, the ratio of sulphates may become very low, probably in part because of the practical arrest of the intestinal contents, and their failure to pass through the intestine. May not this be the explanation of the low ratio of sulphates observed in melancholia, at least in some cases? This interpretation is certainly one that should be borne in mind in considering the origin of excessive intestinal putrefaction in such cases.

The following are the results which have been obtained:

CASE I. *Acute Melancholia.*

DATE.	Preformed sulphates, grammes.	Combined sulphates, gramme.	Ratio.	Urea, grammes.	Indigo blue.
1892.....	0.845	0 251	3.4	13.638	
April 28th.....	0.753	0.206	3.6	11.806	Negative.
May 4th, 1893.....	1.014	0.311	3.3	16.684	Traces only.
May 21st	1.117	0.131	8.5	13.325	Negative.
May 25th.	1.102	0.215	5.1	14.765	"
May 27th.....	0.730	0.119	6.1	9.771	"
May 31st.....	3.051	0.242	12.6	18.096	Traces.
June 3d.....	1.213	0.259	4.6	16.827	"
June 7th.....	1.378	0.424	3.2	20.937	"
June 10th.....	1.036	0.312	3.3	15.929	Negative.

Here the ratios are much too low, and the combined sulphates in some instances above the normal.

[1] Untersuchungen an zwei hungernden Menschen. *Arch. für pathologische Anatomie und Physiologie und für klin. Med.*, 1893–cxxxi–Supplementheft.

CASE II. *Melancholia.*

DATE.	Preformed sulphates, grammes.	Combined sulphates, gramme.	Ratio.	Urea, grammes.	Indigo blue.
May 2d, 1893.....	1.176	0.206	5.7	16.257	Negative.
May 27th.....	1.740	0.201	8.6	20.805	"

Ratio too low, combined sulphates within the normal.

CASE III. *Melancholia.*

DATE.	Preformed sulphates, grammes.	Combined sulphates, gramme.	Ratio.	Urea, grammes.	Indigo blue.
May 3d, 1892.....	1.238	0.272	4.5	18.461	Traces.
May 27th.....	0.695	0.228	3.0	14.083	Negative.

Ratios too low.

CASE IV.—*Melancholia.*

DATE.	Preformed sulphates, grammes.	Combined sulphates, gramme.	Ratio.	Urea, grammes.	Indigo blue.
May 3d, 1893.....	2.622	0.268	9.7	32.344	Strong.
May 27th.....	2.430	0.181	13.4	26.721	"

Ratios normal.

CASE V. (*Dr. Dold's Patient*) *Acute Melancholia* (*with Frenzy*).—Mrs. A. L., aged forty-two years. Mental disorder followed her witnessing the suicide of her husband. Suffers from great mental depression, and makes frequent attempts at suicide. At one time was in a condition of frenzy. Patient is robust and general health is excellent.

DATE.	Preformed sulphates, gramme.	Combined sulphates, gramme.	Ratio.	Urea, grammes.	Indigo blue.	
Nov. 27th, 1893	1.223	0.050	24.4	13.911	Negative.	Partial sample.
Nov. 28th.....	0.776	0.133	5.8	11.658	Very strong purple.	
Nov. 29th.....	1.098	0.158	6.9	12.074	Strong purple.	
Nov. 30th.....	0.883	0.168	5.1	13 845	" "	
Dec. 1st.....	1.052	0.177	6.2	20.064	" "	
Dec. 2d.....	0.902	0.168	6.6	11.416	Medium.	Sediment of urates.
Dec. 3d.....	1.042	0.136	5.7	12.810	Strong purple.	
Dec. 4th.....	0.851	0.082	6.4	12.020	" "	Do.
Dec. 5th.....	0.691	0.097	7.1	7.525	" "	
Dec. 6th.....	0.873	0.135	6.2	9.714	" "	Do.
Dec. 7th.....	0.690	0.070	9.8	9.204	" "	
Dec. 8th.....	0.696	0.194	7.4	7.779	" "	
Dec. 9th.....	1.129	0.144	7.8	14.819	M'dium purple.	
Dec. 10th.....	0.638	0.104	6.1	7.949	" "	
Dec. 11th.....	1.164	0.166	7.0	15.402	Strong purple.	
Dec. 12th.....	1.154	0.131	8.8	9.876	Weak purple.	
Dec. 13th.....	0.651	0.097	6.7	8.516	" "	

CASE VI. (*Dr. Dold's Patient.*) *Acute Melancholia* (*Recurrent*).—Mrs. D., aged forty-seven years. Two previous attacks, from which there was apparently perfect recovery. Present attack began with the delusion that she was the most wicked woman living, and that she had caused all the water to dry up so that people were perishing by millions. Recovery very gradual; sleep bad; general nutrition poor.

DATE.	Preformed sulphates, gramme.	Combined sulphates, gramme.	Ratio.	Urea, grammes.	Indigo blue.	
Dec. 16th, 1893	0 785	0.200	3.9	10.194	Weak.	
Dec. 17th.....	0.374	0.167	2.1	7.980	"	
Dec. 18th.....	0.520	0.169	4.8	.....	"	
Dec. 19th.....	.....	.....	.....	2.340	"	Partial sample.
Dec. 20th.....	0.358	0.090	4 0	5.361	"	" "

CASE VII. (*Dr. Dold's Patient.*) *Acute Melancholia.*—Mrs. C. A. L., aged sixty-six years. Present attack followed financial troubles and the death of her husband. Has had the delusion that she has stolen millions, and will be thrown into prison. Talks of suicide, thinks she is watched, and says she is very wicked. Eats and sleeps poorly.

DATE.	Preformed sulphates, grammes.	Combined sulphates, gramme.	Ratio.	Urea, grammes.	Indigo blue.	
Nov. 14th, 1893	2.231	0.184	12.1	22.624	Very strong.	
Nov. 15th.....	1.432	0.117	12.2	14.116	Traces.	
Nov. 16th, 17th	1.817	0.244	7.4	21 803	Negative.	
Nov. 17th, 18th	2.073	0.208	10.0	20.862	Weak.	
Nov. 18th, 19th	1.120	0.131	8.5	13.704	Negative.	
Nov. 19th, 20th	1.199	0 192	6.2	12.723	"	
Nov. 20th, 21st	1.772	0.204	8.7	17.862	"	Urine alkaline.
Nov. 21st, 22d.	1.670	0.183	9.0	15.343	"	Do.
Nov. 22d, 23d..	1.429	0.140	10.2	13.121	"	Do.
Nov. 23d, 24th.	1.353	0.174	7.8	14.389	"	Do.
Nov. 24th, 25th	1.740	0.140	12.4	14.598	"	Do.

In two cases of acute mania the results were normal.

CASE IX. *Acute Mania.*

DATE.	Preformed sulphates, gramme.	Combined sulphates, gramme.	Ratio.	Urea, grammes.	Indigo blue.
May 4th, 1893.....	1.567	0.164	9.5	20.518	Negative.
May 19th.....	1.554	0.184	8.4	21.226	Medium violet red.
May 21st.....	2.133	0.081	26.3	21.507	Negative.
May 25th.....	1.522	0.161	9.4	19.833	"
May 27th.....	2.357	0.185	12.7	27.286	"
June 7th.....	1.669	0.192	8.7	22.041	"

Ratios normal, combined and preformed sulphates normal.

CASE X. *Acute Mania.*

DATE.	Preformed sulphates, grammes.	Combined sulphates, gramme.	Ratio.	Urea, grammes.	Indigo blue.
June 3d, 1893.....	1.707	0.190	9.0	24.591	Negative.
June 10th.....	2.226	0.216	10.3	28.506	Weak pink.

Ratios and sulphates normal.

Excessive Intestinal Putrefaction in Kidney Disease.—The fact that chronic nephritis is commonly associated with an excess of the products of intestinal putrefaction was first noted by Biernacki[1] in 1891. In his studies, Biernacki used typical cases of chronic nephritis, uncomplicated, and said to be free from symptoms of intestinal disorder of any kind. In his carefully studied cases, the ethereal sulphates were present in greater amount, at least as compared with the preformed sulphates, than in health, and the conclusion is reached that such excess is one of the features of chronic nephritis. The interest of Biernacki's observations is heightened by the fact that, while studying the aromatic sulphates, he made observations also upon the hydrochloric acid of the gastric juice in his cases of nephritis, and found it to be distinctly decreased in amount. Moreover, there seemed to be a relation between these two classes of findings: when the excretion of putrefactive products was large, there was a greater decrease in the amount of HCl secreted, and greater evidence of gastric disorder than when the aromatic sulphates of the urine were not appreciably increased.[2] It is, of course, very tempting to go a step farther, and conclude that the relation just mentioned is to be explained by the fact that the

[1] Ueber die Darmfäulniss bei Nierenentzündung und Icterus. *Deutsches Archiv für klinische Medicin*, 1891-lxix-Heft 1.

[2] Some of Biernacki's figures are appended.

gastric juice normally exerts a powerful influence in checking intestinal putrefaction, while the deficiency in HCl removes this check. And it cannot be denied that this absence of HCl may be an important factor in determining an excess of the aromatic sulphates. But the origin of such excess is probably more complex than this, and we are not even fully assured that the HCl of the gastric juice really plays so important a rôle as has been ascribed to it in checking intestinal putrefaction. One of the best experimental evidences of its action is that brought forward by Kast,[1] who showed that the neutralization of the gastric juice by sodium carbonate, was followed by an increased excretion of the ethereal sulphates. Yet, as von Noorden has indicated, it is by no means certain that this result is the consequence of neutralization of the acidity of the gastric juice. The action of the alkali is not limited to the stomach, and its direct effect upon the intestinal contents may be such as to favor putrefaction there. Then, again, we have to note the facts which we have observed, that the neutralization of the gastric juice by alkalies, may have no appreciable effect on putrefaction, and that the use of HCl by the stomach may have no influence in decreasing putrefaction.

As will be seen by the figures given in connection with the following cases of nephritis, which were kindly put at our disposal by Dr. Northrup and Dr. Hedges, the increased excretion of aromatic sulphates observed by Biernacki was present in a number. In two of the cases (those of I. O. and R. H.) the ratios are such as belong to health, but inasmuch as all the cases were on a milk and broth diet—that is, on a diet which normally reduces the ratio to 1 to

[1] Ueber die qualitative Bemessung der antiseptischen Leistung des Magensaftes. *Zeitschrift zur Eröffnung u. s. w.*, Hamburg, 1889, S. I. See also Wasbutzki, *Archiv für experiment. Pathologie und Pharmakologie*, Bd. xxvi, S. 133; and Stadelmann, *Ueber den Einfluss der Alkalien auf den menschlichen Stoffwechsel*, 1890.

15, or 1 to 20—we may at least question the normal character of the ratio. In the remaining cases, there is no doubt of the excess of aromatic sulphates. In one of the cases of acute nephritis, the excess is particularly striking. We may conclude that some form of excessive intestinal putrefaction is a common occurrence in nephritis. Much more extended observation is necessary to determine what forms of nephritis, if any, are especially apt to be thus complicated.

Patient, aged thirty-two years; ill three weeks. Free diuresis for three days. Acetate of potassium for the last time on the 6th. No drugs since that date. Urine contains abundant albumin. Greatly diminished free HCl. Hæmaturia.

DATE.	Preformed sulphates, grammes.	Combined sulphates gramme.	Ratio.	
16th.....	4.4235	0.6936	6.5	
18th.....	2.2274	0.4816	4.6	
20th.....	2.1454	0.3740	5.7	Milk diet.
21st.....	1.7647	0.6897	4.7	
23d.....	1.6668	0.6048	2.7	
24th.....	2.3480	0.3460	6.7	
25th.....	2.3832	0.2772	8.5	HCl after meals.
26th.....	1.7232	0.4392	3.9	
28th.....	2.8840	0.3480	8.2	Middle diet: proteids, 59.8 grammes; fat, 34.5 grm.
29th.....	3.9420	0.3780	10.4	Carbohydrates, 26.3 grm. daily.
30th.....	2.0010	0.4920	4.0	HCl discontinued.
31st.....	2.0160	0.5805	3.4	
	2.4455	0.5681	4.3	Average from 16th to 23d.
	2.6560	0.3576	7.4	Average from 24th to 29th; HCl after meals.
	2.0085	0.5145	3.9	Average without HCl.

Patient, aged twenty-eight years; ill two months. Free diuresis for one week. Free HCl usually absent, weak traces on the 23d and 31st. Bowels regular. Urine contains much albumin. Hæmaturia diminishing since the 24th.

Date.	Preformed sulphates, grammes.	Combined sulphates, gramme.	Ratio.	
6th	1.012	0.2448	4.1	Milk diet. HCl begun.
7th	2.460	0.4165	4.8	
8th	1.1136	0.3216	3.4	
9th	1.0980	0.7290	1.5	
10th	1.7062	0.4978	3.4	
11th	1.9080	0.4200	4.5	
13th	1.3936	0.1482	9.3	
14th	1.1126	0.0942	11.8	
15th	1.9392	0.1776	10.9	
17th	1.6740	0.1860	9.0	
18th	2.0553	0.4335	4.7	HCl stopped.
22d	1.7450	0.3200	5.4	
23d	2.6300	0.4100	6.4	
24th	2.6376	0.4872	5.4	Ordinary diet; proteids, 95.3 grammes; fat, 66.9 grammes; carbohydrates, 477.7 grammes.
25th	2.3954	0.2031	11.8	
26th	1.6160	0.1240	13.0	
27th	2.5530	0.1150	22.2	
28th	1.7472	0.1176	14.8	HCl used.
29th	2.5960	0.2200	11.8	
	1.4804	0.4882	3.3	Average, 6th to 11th, without HCl.
	1.5298	0.1515	10.1	Average, 13th to 17th, with HCl after meals.
	2.6669	0.4127	6.4	Average, 18th to 24th, without HCl.
	2.1875	0.1558	14.0	Average, 25th to 29th, HCl after meals.

These tables illustrate clearly the low ratio of sulphates seen in Bright's, and the influence of HCl in modifying this ratio by checking the formation of putrefactive products eliminated as ethereal sulphates.

Case I.—I. O., aged thirty-four years; weight, one hundred and forty pounds. Moderate alcoholic habit. Present trouble began several months ago, after exposure to cold, with œdema of the lower extremities and scanty urine. Now there is great œdema of the lower extremities, and abundant urine containing considerable albumin. Constipation, good appetite, no apparent anæmia. Diet: milk, broths, and a little bread.

DATE.	Preformed sulphates, grammes.	Combined sulphates, gramme.	Ratio.	Indigo blue.	Urea, grammes.
Nov. 28th.....	1.923	0.171	11.2	Weak.	25.480
Dec. 1st.....	1.787	0.137	13.0	Strong.	18.214
Dec. 2d.....	2.348	0.077	30.5	"	

CASE II.—R. H., aged thirty-eight years; weight, one hundred and forty pounds. Has had several attacks of articular rheumatism, and for several years has been troubled with dyspnœa. About seven months ago began to have œdematous extremities, and scanty, sometimes bloody, urine. At present is weak, has œdematous extremities, dyspnœa, enlarged heart, with systolic murmur at base, and scanty urine, with considerable albumin and abundant casts. Diet: milk and broths.

DATE.	Preformed sulphates, gramme.	Combined sulphates, gramme.	Ratio.	Indigo blue.	Urea, grammes.
Nov. 19th.....	0.911	0.078	11.7	Medium.	
Nov. 20th.....	0.1185	0.103	11.5	Weak.	
Nov. 21st.....	0.911	0.065	14.0	Medium strong.	
Dec. 1st.....	0.1082	0.126	8.6	Weak.	18.249

CASE III.—G. V., aged forty years; weight, two hundred pounds. For two months has had œdematous extremities, dyspnœa, feeble heart action, and rather scanty urine. Albumin in small amount, granular casts abundant. Milk diet. Bowels regular, appetite fair, no constipation.

DATE.	Preformed sulphates, grammes.	Combined sulphates, gramme.	Ratio.	Indigo blue.	Urea, grammes.
Nov. 21st.....	1.257	0.208	6.1		
Nov. 21st.....	2.154	0.250	8.6	.....	16.85

CASE IV.—H. G., aged forty-two years; weight, two hundred and seventeen pounds. Rheumatic and alcoholic history. Duration of present trouble, two years. At present, general anasarca, dizziness, constipation, dyspnœa, and feeble heart action. Urine scanty, albumin moderate in amount. Diet: milk and broths.

DATE.	Preformed sulphates, gramme.	Combined sulphates, gramme.	Ratio.	Indigo blue.	Urea, grammes.
Nov. 13th.....	1.236	0.184	6.7	.....	17.162
Nov. 14th.....	1.599	0.225	7.1	....	22.630
Nov. 15th.....	1.752	0.164	10.7		
Nov. 16th.....	1.639	0.194	8.4		

CASE V.—N. C. S., aged forty-nine years; weight, one one hundred and thirty pounds. Moderate alcoholic habit. Present trouble dates back five years, when she began to suffer from headache, dizziness, swelling of extremities, dyspnœa, and palpitation of heart. Urine, low specific gravity, and abundant. Albumin in moderate amount. Diet chiefly milk.

Date, Dec. 17th.

Preformed sulphates.....	0.537 gramme.
Combined sulphates.....	0.119 "
Ratio.....	4.5
Indigo blue.....	Strong.
Urea.....	10.886 grammes.

CASE VI.—J. S., aged twelve years; weight, ninety-six pounds. Family history of Bright's disease. Personal history, scarlet fever. For four months has suffered from œdema of feet, ankles, and eyelids, headache, and moderate anæmia. Appetite fair, bowels regular. Albumin abundant in urine; quantity of urine about normal. Diet: milk, bread, and meat once daily.

DATE.	Preformed sulphates, gramme.	Combined sulphates, gramme.	Ratio.	Indigo blue.	Urea, grammes.
Nov. 13th.....	1.612	0.168	9.6	Negative.	19.884
Nov. 14th.....	1.332	0.168	7.9	Medium strong.	17.759
Nov. 15th.....	1.309	0.109	12.0	Weak.	
Nov. 16th.....	1.308	0.161	8.7	Very strong.	

CASE VII.—M. B., aged nineteen years. Two years ago was exposed to cold and wet, and afterward had œdema of face and extremities, an anasarca, with bloody and scanty urine. Has not been well since. At present, has œdematous extremities, ascites, scanty urine, headache, frequent and sometimes severe vomiting, and loss of appetite and failing sight.

DATE.	Preformed sulphates, gramme.	Combined sulphates, gramme.	Ratio.	Indigo blue.	Urea, grammes.
Jan. 8th.....	.....	0.051	.....	Negative.	4.983
Jan. 9th.....	0.536	0.078	6.9	"	7.587
Jan. 13th.....	0.273	0.091	3.0	"	6.674

CASE VIII.—C. T., aged thirty-two years. Family history of Bright's disease, and bad personal alcoholic history. Present trouble dates back several months. At present, slight general anasarca, dizziness, weakness, numbness, paræsthesia of legs. High-tension pulse. Urine scanty. Albumin in small amount, and granular casts abundant. Milk diet.

DATE.	Preformed sulphates, gramme.	Combined sulphates, gramme.	Ratio.	Indigo blue.	Urea, grammes.
May 18th.....	0.224	0.145	1.5	Strong.	10.065
May 19th.....	0.306	0.185	1.6	Medium.	10.299

CASE IX.—J. C., aged sixty years. For about two years has had occasional swelling of feet, headache, and gradually increasing loss of vision, due to neuro-retinitis. Is now without gastric or intestinal symptoms on a mixed diet; has moderate albuminuria, and is passing a large amount of urine, which contains hyaline and granular casts.

DATE.	Preformed sulphates, grammes.	Combined sulphates, gramme.	Ratio.	Indigo blue.	Urea, grammes.	
Nov. 30, 1894..	1.404	0.223	6.3	Weak.	18.469	Milk diet.
Dec. 10th.....	1.406	0.276	5.1	.....	16.630	
Jan. 8, 1895....	1.347	0.352	3.8	Negative.	19.775	

Excessive Intestinal Putrefaction in other Pathological Conditions.—Besides the pathological conditions already mentioned, in which excessive intestinal putrefaction has appeared to constitute an important and frequent feature, there are undoubtedly others with which it is often associated, but in which it has not yet been observed with enough frequency and regularity to enable us to say with confidence whether its occurrence is characteristic. Thus Biernacki[1] has found a marked increase in the ethereal sulphates in catarrhal jaundice, but, though his cases were very carefully studied and gave positive and unequivocal results, they were too few—being only three in number—to enable us to draw any general conclusion. Nevertheless, Biernacki's observations are of sufficient interest for us to briefly review them. In choosing his cases for study, he excluded jaundice arising from cirrhosis, cancer of the liver, or from impacted gallstones. He tried also to rule out cases in which intestinal catarrh was present, recognizing that this is apt, by itself, to increase putrefaction. In the interpretation of his results, Biernacki makes due

[1] *Loc. cit.*, p. 109.

allowance for the influence of different kinds of food—a matter of cardinal importance. His observations relate to the aromatic sulphates; no estimation of indican was made.

Biernacki attributes the increased putrefaction observed largely, but not altogether, to the mechanical holding back of the bile from the gut—the idea being that the bile exerts normally a pronounced antiseptic effect on the intestinal contents. The chief fact which he brings forward in support of the view is that the ethereal sulphates diminished in amount just at the time of beginning convalescence, when the bile pigments disappeared from the urine, and when, presumably, the bile had begun to find its way once more to the intestine. But while these sulphates are thus reduced, at times abruptly, with the disappearance of the bile pigments from the urine, there still remains an excess of ethereal sulphates until convalescence is completed. This excess, Biernacki thinks, cannot be attributed to the absence of the bile from the intestine, but rather to those anomalies of gastric secretion (especially the deficiency in HCl which has been found by some observers in jaundice) which accompany the gastritis which ordinarily coexists with catarrhal jaundice. To what extent the shutting off of the bile is to be regarded as a factor in the production of intestinal putrefaction is not at present clear. What antiseptic action the bile may possess doubtless depends on the free biliary acids. It has no such action in an alkaline medium, and little or none in a neutral medium. How much antiseptic action the bile has in an acid medium, we do not know. According to Lindberger[1] and Leubuscher[2] this action is considerable, but according to Macfadyen, whose experiments were carried out with much care, it is slight. Macfadyen contends that the bile has no specific

[1] Abstract in Virchow and Hirsch's *Jahresb.*, 1884, i, p. 141.

[2] Einfluss von Verdauungssecretion auf Bakterien, *Ztschr. f. klin. Med.*, 1890 xvii-481.

antiseptic powers, and that such action as it possesses depends on the bile acids and is shared by acid media in general. Von Noorden points out that the quantity of the free bile acids is usually small, and that its action may be minimized by the presence of albumin and peptone. He suggests that a much more potent factor in the prevention of putrefaction in the small intestine is to be found in the organic acids that originate in the fermentation of carbohydrates. It is probably safe to conclude that the bile possesses only slight antiseptic properties, and that its influence in controlling putrefaction in the intestine depends largely on its hurrying the passage of food, and thus preventing the formation and absorption of aromatic substances. The high ethereal sulphates observed by Biernacki in jaundice were also found by Brieger.[1] Brieger found no excess of indol, phenol, or skatol, and concludes that there are unknown aromatic substances which combine with sulphuric acid in the organism, and that these are increased in jaundice and, perhaps, account for the peculiar stink of bile-free fæces.

Müller,[2] Pott,[3] and von Noorden[4] have observed cases of obstructive jaundice in which no increase in the ethereal sulphates could be detected. We also obtained quite negative results in two imperfectly studied cases of catarrhal jaundice.

Senator[5] was the first to call attention to the fact that in cases of pronounced anæmia the urine generally contains considerable indigo blue. Since this observation, which has been repeatedly confirmed, others have found an increase in the ethereal sulphates in pernicious anæmia, leukæmia, and anæmic conditions generally. Von Noorden

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[1] *Ztschr. f. klin. Med.*, 1881, 3, p. 465.
Untersuch. über Ikterus, *Ztschr f. klin. Med.*, 1887, xii–45.
[2] Pflüger's *Archiv*, 1890, xlvi–19.
[4] *Berlin. klin. Woch.*, 1892.
[5] *Ctrlbl. f. d. med. Wiss.*, 1877, pp. 357, 370, 388.

and Rethers[1] have observed an increase in the ethereal sulphates in many cases of chlorosis. In eighteen cases of moderate or pronounced anæmia, Rethers found no increase in the ethereal sulphates in nine cases, but in the remaining cases he found a considerable, though not constant, excess. We have had a similar experience with a smaller number of cases of simple anæmia.

These observations are of interest in connection with the view of Sir Andrew Clark,[2] that anæmia results from the absorption from the intestine of substances harmful to the blood that are formed in consequence of intestinal putrefaction. It is clear from Rether's results that this cannot be the manner in which most anæmias are produced, but the possibility remains that some anæmias may arise in that way. But the work done upon the occurrence of excessive intestinal putrefaction in the various blood diseases is at present unsatisfactory, and we can hardly say more than that many anæmias are complicated, either continuously or irregularly, by intestinal disturbances which vary considerably in different cases and require to be individually studied.

In three cases which were diagnosticated ulcer of the stomach, we found regularly very high ethereal sulphates, both relatively and absolutely. One of the cases proved on autopsy to be carcinoma of the stomach. While the results in these cases were striking, they are inconclusive. It is possible that the lack of gastric secretion, in both ulcer and cancer of the stomach, might account for excessive intestinal putrefaction. Further studies of these conditions are desirable. Our results in two cases are as follows:

CASE I.—J. C., aged nineteen, domestic, single. Family and previous history good. Has not menstruated for two years. Present trouble began about six months ago,

[1] *Beitr. zur Pathol. der Chlorose*, Diss. Berlin, 1891.

[2] Anæmia and Chlorosis of Girls. *Lancet*, 1886, ii, 1003.

with pain in epigastrium, headache, palpitation and shortness of breath, and some œdema of extremities. Two weeks ago, vomited blood for first and only time. Now complains of much pain in epigastrium, shooting through to small of back, and made worse by eating. Patient is well nourished, very anæmic, tongue is coated; hæmic bruit at base of heart; stomach dilated; extremities slightly œdematous; hæmoglobin, twenty per cent.; stools, dark and tarry; weight, a hundred and twenty pounds.

Date.	Preformed sulphates, grammes.	Combined sulphates, gramme.	Ratio.	Indigo blue.	Urea, grammes.	
Jan. 6, 1893...	1.483	0.233	6.3	Very strong.	18.342	
Jan. 9th.....	1.457	0.357	4.1	Strong.	27.633	
Jan. 10th.....	0.583	0.167	3.5	Traces.	15.749	
Jan. 11th.....	0.920	0.199	4.6	"	17.765	Improved condition.
Jan. 14th.....	0.734	0.212	3.4	Negative.	15.578	
Jan. 30th.....	1.097	0.155	7.0	"	15 425	

Case II.—J. E., aged forty-six, carpenter, married. Family history good. For last four months, has had poor appetite, and has lost flesh and strength. Pain after eating is a regular occurrence, and there has been occasional vomiting of blood. On admission to hospital, patient was extremely anæmic; hæmic bruit over base; hæmoglobin, twenty-two per cent.; red cells, forty-one per cent.; ratio of red and white cells, normal. Gunzberg's test shows presence of free HCl in stomach washing after test meal. Stools, dark and tarry at first; weight, a hundred and fifty pounds.

Date, Dec. 11, 1892.

Preformed sulphates.....	1.628 gramme.
Combined sulphates.....	0.674 "
Ratio.....	2.4
Indigo blue.....	Medium.
Urea.....	28.090 grammes.

In two cases of Basedow's disease, decided evidences of excessive intestinal putrefaction were found, and it is not improbable, as pointed out by some authors, notably Thompson, that digestive disorder is usually a factor in

the origin of this condition, though our observations prove nothing, as they may be merely due to an accidental association. In both these cases, there were no symptoms of disordered digestion of which the patients were conscious.

In one case of cystinuria which we have, through the kindness of Dr. Thacher, had an opportunity of studying with care, there was regularly a considerable increase in the amount of ethereal sulphates excreted, notwithstanding the patient was on a milk diet, this excess disappearing as the patient became convalescent.[1] This fact is of interest in connection with the view that cystine—a diamine—is a product of intestinal putrefaction. It must be stated, however, that some observers have failed to find, in cases of cystinuria, any increase in the aromatic sulphates.

It was pointed out, many years ago, by Senator, that in conditions where there is extensive suppuration, there is usually to be found a large amount of indican in the urine. Recently, Keilman[2] put forward the view that indican is regularly to be found in the urine whenever abscess formation is taking place, and that the presence of indigo blue, under certain conditions, may be of much aid in the recognition of obscure abscesses. In several cases, the indican disappeared from the urine as soon as the abscess had been opened and drained.

We are unable to offer any criticism on the statement of Keilman that abscess formation is regularly associated with the occurrence of indican in the urine. In two cases of tubercular bone disease, with large, freely suppurating cavities, indigo blue was observed to be of inconstant occurrence in the urine; but we cannot admit this fact in evidence here, since in these cases the pus was not con-

[1] We have recently found a marked increase of the ethereal sulphates in a second case of cystinuria, occurring in a boy aged five years.

[2] *St. Petersburger med. Wochenschrift*, No. 15, 1893.

fined, but had free exit. We may, however, call attention to the difficulty that must attend the employment of indicanuria in the diagnosis of abscess—a difficulty apparently overlooked by Keilman. This is due to the fact that indican is so frequently present in the urine as a result of intestinal derangements in which symptoms are obscure or absent. Consequently, before referring indigo blue to the putrefactive products formed in abscesses, it would be necessary to rule out with certainty the existence of excessive intestinal putrefaction with the production of indican. It is unnecessary to say that this is usually difficult and often impossible. A similar criticism applies to the use of indican as an evidence of the existence of tuberculosis. Some authors, however, appear to regard the occurrence of considerable indican in the urine in children from one to six years of age as evidence of tuberculosis. Recent observers, especially Steffen and Voute,[1] have shown that the facts upon which this view is based are themselves open to question, and that indicanuria is no more common in tubercular children than in children without tuberculosis. We shall not enter here into a discussion of the merits of Hochsinger's views upon this question, and shall only point out the fact that the statements already made regarding the origin and nature of indigo blue, render it in the highest degree improbable that indigo blue found in the urine of tubercular children is due to the presence of a tubercular process. But we can readily understand how indigo blue in the urine might be of frequent occurrence in tubercular children who have intestinal disorders, and it is conceivable that where a tubercular process has led to extensive suppuration this may contribute to the formation of indican.

It is probably not out of place to note here the behavior of the aromatic substances of the urine during inanition. At first sight, it might be supposed that the putrefactive

[1] Voute. *Revue mens. des maladies de l'enfance.*

processes in the intestine would be greatly reduced by the absence of proteid material capable of undergoing decomposition. Observation, however, shows that the putrefactive products not merely do not cease to be formed in starvation, but may be actually increased.

The best work that has yet been done upon the ethereal sulphates in starvation is that of Müller,[1] whose results upon two starving subjects, Cetti and Breithaupt, are given below:

	CETTI.			BREITHAUPT.		
	Preformed sulphates, grammes.	Combined sulphates, gramme.	Ratio.	Preformed sulphates, grammes.	Combined sulphates, gramme.	Ratio.
Second day before experiment	.....	.....	.....	2.586	0.320	8.8
Day before experiment begun	2.790	0.185	14.5	2.606	0.250	10.4
First day without food	2.094	0.060	36.5	2.156	0.398	5.6
Second day	1.777	0.043	40.9	1.800	0.336	5.3
Third day	1.874	0.064	29.3	2.235	0.276	8.1
Fourth day	1.907	0.104	18.3	2.029	0.239	8.5
Fifth day	1.665	0.160	10.4	1.927	0.135	14.2
Sixth day	1.607	0.136	11.8	1.546	0.216	7.1
Seventh day	1.356	0.259	5.2			
Eighth day	1.316	0.267	4.9			
Ninth day	0.851	0.389	2.3			
Tenth day	1.252	0.166	7.4			
First day on food	2.107	0.267	7.8	1.840	0.387	4.8
Second day on food	3.162	0.575	5.5	1.747	0.077	22.7

Thus in the case of Cetti, there was a reduction of the ethereal sulphates, and a slighter reduction in the preformed sulphates during the first four days. Then, while the preformed sulphates continued to fall gradually, the combined sulphates underwent a marked increase, the ratio the while growing low. On the two days following the resumption of food, the preformed sulphates rose to

[1] Untersuchungen an zwei hungernden Menschen. *Arch. für pathologische Anatomie und Physiologie*, 1893–cxxxi, Supplement, p. 128.

normal, and the ethereal sulphates continued excessive. In the case of Breithaupt, the results are less striking. There the preformed sulphates underwent less reduction in the starvation period than in the case of Cetti, and the ethereal sulphates did not rise much above the figures of the normal days preceding. Hence, the ratios did not fall to the point recorded in Cetti's case.

While it would have been better had both cases been studied for a longer time, before and after the period of starvation, these results illustrate well the increase in the products of intestinal putrefaction that may occur. We know of no other observations upon sulphates in starving men. Some results obtained by us in starving rabbits show a close correspondence with the conditions found in Cetti and Breithaupt's cases—namely, a reduction in the preformed sulphates, and an increase in the aromatic sulphates. When food was given, the ratios returned rapidly to the normal.

As regards the formation of indigo blue during inanition in man, there is now little difference of opinion. In the two cases just referred to, Müller found that indigo blue was present only in traces during the period of starvation, though it had been present in large amount before, and that it reappeared in increased amount when food was again taken. Tuczek[1] also found that indigo blue disappeared from the urine of an insane patient who abstained entirely from food. In this case, the indigo blue did not return until the fourth day upon food. Luciani, in the case of Succi, observed a reduction in ethereal sulphates to about one third that found on a mixed diet. It thus seems, from this rather scanty material, that we are likely in starving persons to meet with a reduction or disappearance of indigo blue, and this notwithstanding there may be coincidently an increase in the ethereal sulphates. But

[1] Stoffwechhsel bei abstinirenden Geisteskranken. *Arch. f. Psychiatrie*, 1884–xxv–784.

it is clear, from a case recorded by Müller,[1] that no reduction in indigo blue may occur in starvation, if the patient is suffering from gastro-intestinal disease.

In the case of starving animals, the results regarding indigo blue are conflicting. Thus Salkowski[2] found seventeen milligrammes of indigo in the urine of a dog on a meat diet, and only from four to five milligrammes during a period of starvation. In this instance, therefore, there was merely a reduction in indigo blue. Other observers have found that indigo blue may persist in considerable amount during inanition in cats. We have ourselves noted in rabbits the appearance of considerable amounts of indigo blue during starvation (although none had been present previous to the shutting off of food) and its entire disappearance when food was given again.

It would not be unreasonable to expect that in starving men, the output of phenol, as phenol-potassium sulphate, should, like indoxyl-potassium sulphate, undergo a decrease.

But the observations of Munk,[3] Müller,[4] and Senator[5] show that this is by no means the case. The results of Müller in the case of Cetti may probably be regarded as typical. Here and there was a reduction in the excretion of phenol during the first day of fasting, followed by a gradually increasing output, until the amount reached a hundred and thirty-seven and a hundred and fifty-five milligrammes on the eighth and ninth days respectively of the fast—that is, from three to seven times as much phenol as is excreted on the average upon a mixed diet.

It is thus probably safe to say that in fasting human beings, there is not merely an increase in intestinal putrefaction, as expressed by the appearance of the ethereal sul-

[1] The case referred to was one of œsophageal stricture, following alkali poisoning, in which the digestive tract was also involved.

[2] Ueber die Quelle des Indicans im Harn des Fleischfressers. *Ber. chem. Ges.*, 1876, ix-138.

[3] *Loc. cit.* [4] *Loc. cit.* [5] *Loc. cit.*

phates in increased amounts, but a change as well in the *nature* of the putrefactive process, as expressed by the decrease of indol formation, and the increase in phenol formation. These results, as already intimated, are not what might be expected on a casual view of the origin of the putrefactive processes in the intestine. The increase in the ethereal sulphates in starvation is, however, less surprising when we reflect upon the character of the contents of the intestine during this condition. In addition to the remnants of proteid food which the intestine then contains, there is a rich basis for putrefactive processes in the nucleo-albumins, and the mucin of the bile,[1] and of the succus entericus, and in the proteid constituents of the broken-down epithelial lining of the gut. The absence or feeble character of the peristaltic movements during a fast, leaves these various contents in a condition favorable to bacterial activity. The deviation from the normal in the nature of the putrefactive processes, which has been referred to, is probably explained by the unusual character of the material which undergoes putrefaction. Why it is phenol which is increased under these circumstances, we do not know.

In view of the increase in phenol observed in the fasting state, it is of interest to note that such an increase is not, according to Munk,[2] found in conditions of mere constipation. It is interesting, also, in seeking to explain the fact that putrefaction is increased in starvation, to recall the influence which certain articles of food are supposed to exert normally in inhibiting putrefactive processes. Thus, according to Hirschler,[3] cane sugar, starch, and dextrine, and their derivative, lactic acid, exert some influence of this nature. The same is thought to be true of the glycerin formed in consequence of the proteid decomposition of food.

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[1] In this connection, see the observations of Ernst, which show that aromatic bodies arise from putrefaction of the mucin of the bile. *Loc. cit.*

[2] *Loc. cit.* p. 133.

[3] *Zeitschr. f. physiol. Chem.*, Bd. x, p. 306.

While it is quite conceivable that these foods exert in a measure the influence ascribed to them, we are in no position to assert that they do.

The view of Ortweiler,[1] that hæmorrhages into the intestine and other pathological conditions of the gut in starvation, may be factors in producing increased putrefaction, can not be denied, but it is unlikely that they are ordinarily factors of importance.

It is important to understand that excessive intestinal putrefaction is sometimes met with in persons who give no symptoms of any kind, or only obscure symptoms, and are to be regarded as normal. We have met with several instances of this kind where the sulphates have been habitually a little high (say about one to eight), and where a moderate amount of indican has been present. Such cases are certainly not common. But how are we to explain the absence of symptoms, or their slight character? The explanation lies, probably, in the fact that certain individuals are so robust in general health, and so resistant to the influence of such putrefactive products as are formed in the intestine, that such substances cause no appreciable effect. This corresponds with the fact that the individuals in whom we have observed this behavior, in the presence of abnormal conditions, have been, without exception, in unusually strong general health. Possibly also in them the disorder has been of short duration. We are, therefore, not to interpret such cases as evidence that high sulphates and indican are ever to be regarded as normal occurrences, but rather, that in some exceptionally robust persons, they produce, for a time at least, little or no appreciable effect. An analogous condition is observed in some resistant persons who excrete a considerable excess of uric acid without, for a time, having any appreciable symptoms. The same degree of excess

[1] Physiol. und pathol. Bedeutung des Harnindicans. *Mitth. aus der Würzb. med. Klinik*, 1885-ii, 172.

in less robust persons, is regularly associated with distinct symptoms.

In no instance have we met with an absence of symptoms where the amount of indican was regularly large, or where the sulphates showed a higher ratio than one to eight, or one to seven.

We should mention here the influence of sexual excess in giving rise to excessive intestinal putrefaction. While, from the nature of the case, it is most difficult to make definite observations upon the influence of sexual excess, there can be no doubt that in many males it increases, at least temporarily, the putrefactive substances formed in the intestine, and that habitual excess is a frequent cause of intestinal indigestion. Many patients with chronic intestinal indigestion notice that after copulation the stools are altered in color and consistence for several days, becoming lighter in color and weight. In some cases, indican has been noted temporarily, both after copulation and after masturbation. These effects depend, probably, on the deficiency of the digestive juices after sexual excitement. We are not now in a position to estimate the precise influence of sexual excess under different conditions, but there can be little doubt that it is a factor of much importance in the causation of intestinal indigestion, especially in males. Probably sexual excitement is especially harmful during bodily fatigue, and in connection with over-eating.

The Treatment of Excessive Intestinal Putrefaction—If we admit that the presence of putrefactive products in the urine in increased quantities, is evidence of the occurrence in the intestine of an excessive degree of putrefaction, perhaps associated with a change in the kind of putrefaction, and if we admit, further, that such excess of putrefaction is frequently, if not always, related to or associated with symptoms, it is plain that any treatment directed to this condition should contemplate the reduction of the putrefactive processes in the intestine.

The object of treatment is not to stop all putrefaction—to render the intestine free from the presence of bacteria—but rather, to bring the putrefactive changes which may advantageously take place in the advanced stages of intestinal digestion within the limits of the normal, as indicated by the output of putrefactive substances in the urine. And, while the first-named object, of checking all putrefaction, is entirely impracticable, the latter object, of limiting putrefaction, is one which we may undertake, in most cases, with a reasonable hope of success. The means at our command for the accomplishment of this end, comprise: first, drugs; and second, hygienic measures, especially of a dietetic nature.

The drugs which most naturally suggest themselves for use, in diminishing excessive output of the sulphates, are those thought to be capable of stopping, or restraining, the growth of putrefactive bacteria in the intestine—the so-called intestinal antiseptics. There seems to be a certain fascination about the thought, that when we have loaded a patient's intestine with antiseptics, we have not merely done our duty, but have acted in accordance with the most advanced teachings of science. There are many practitioners who appear to have implicit faith in such treatment in most gastro-intestinal disturbances. But there are others, of equal experience, and probably greater thoughtfulness, who are inclined to question whether this treatment is either rational or effective, and there are some who go so far as to deny to it any efficiency whatever. It must be admitted that, on *a priori* grounds at least, there is not much to be said for the treatment. All effective antiseptics are irritants in some degree, and there is a limit, usually soon reached, to the amount of an antiseptic drug that can be put into the stomach without inducing pain, nausea, and vomiting, or the evidence of poisoning from absorption. For anything like effective disinfection of the intestinal tract, it is probable that the even diffusion of the drug,

through the gut contents, would be essential. Yet such diffusion in adequate strength is not to be expected from the introduction of small amounts of a drug at infrequent intervals. It is likely that what we ordinarily do, in our attempts to check putrefaction in the intestine by means of drugs, is to distribute small, more or less thoroughly disinfected spots at intervals along the intestinal tube, the contents at the intervening places remaining unaffected.

How do these *a priori* notions accord with our practical observations? We know that, while intestinal antiseptics fall short of high expectations, they appear at times to do some good. We cannot, however, measure this effect accurately by clinical criteria. Fortunately, we have at once an accurate and fair means of estimating their effect through the study of their influence upon the excretion of the ethereal sulphates. Since the quantity of these substances varies with the extent of putrefaction in the intestine (with few exceptions), a material reduction in their excretion, coincidently with the use of an antiseptic drug, may be fairly regarded as evidence that putrefaction is being inhibited, whatever may be the effect upon the symptoms. Similarly, the absence of any influence upon the sulphates is evidence that the drug is ineffective. What has the study of the matter from this point of view taught us? We have made numerous observations upon several drugs, including hydronaphthol, beta-naphthol, salicylate of sodium, salol, and resorcin. It might be expected that definite conclusions would have been reached with regard to these drugs, but the difficulties of the study were such as to render this impossible. In the case of salol and resorcin, it was found that these drugs are eliminated as ethereal sulphates, and hence they mask any effect which they may possess in reducing the aromatic products—an unfortunate fact from the standpoint of the investigator.

Judging from clinical evidence, however, we have no doubt that both these drugs, and especially salol, exert a

decided effect upon putrefaction in the intestine. Many of the symptoms of intestinal indigestion (pain, flatulence, diarrhœa) are often rapidly relieved by the use of a two-grain dose of salol every hour. Though salol was employed in several cases in which there was an habitual presence of indican, no effect was observed in reducing it.

Both hydronaphthol and beta-naphthol were studied in several cases. Neither drug, in doses up to six grains three times a day, appeared to have any decided effect in checking the amount of the ethereal sulphates excreted. In larger doses, they are usually not well borne. We may say without hesitation, that in the doses first mentioned neither of these drugs is to be considered an efficient intestinal antiseptic in adults.

Salicylate of sodium, in doses of fifteen grains three times a day, may safely be recommended as a valuable intestinal antiseptic. In several observations made with it, it showed a decided effect in bringing down the excretion of the ethereal sulphates toward the normal. In some cases, also, it exerted a marked influence in reducing the amount of indigo blue. The only objection to its use in the above-mentioned amounts is the irritation to which it gives rise in a good many cases. Some persons have taken it for years in these amounts, with only an occasional short intermission, and have had little to complain of. Others cannot take five grains three times a day without untoward effects. In any case, in adults, where the amount is reduced below ten grains three times a day, salicylate of sodium is not likely to be of much service as an intestinal antiseptic. In the cases of epilepsy that are benefited by the reduction of intestinal putrefaction, salicylate of sodium has appeared distinctly more useful in modifying the frequency of seizures than any other drug mentioned.[1]

[1] Since this was written, we have made use of borax, in doses of five grains three times a day, with marked results in some cases of epilepsy (in conjunction

It must be owned that an effective intestinal antiseptic (using the word effective when the sulphates are appreciably reduced or brought within normal limits), which is generally useful and reasonably free from undesirable effects, remains a desideratum—one, we suspect, which will not be readily supplied.

In 1882, Wassilieff pointed out that calomel possesses antiseptic properties, and Baumann employed calomel in the belief that it is an antiseptic as well as a cathartic, in his classical observations upon the ethereal sulphates. He found, when he gave starving dogs a gramme of calomel for three or four days consecutively, that the urine remained free from ethereal sulphates—an indication that intestinal putrefaction had ceased. In view of this fact, the inference might easily have been drawn that calomel possesses decided antiseptic properties. But Morax found that moderate doses of calomel did not exert any appreciable effect upon the ethereal sulphates, and that they did not disappear except when enough calomel was given to produce copious watery stools. A still more recent observer, Stieff, found that doses of calomel as large as three tenths of a gramme (daily?) had no influence whatever in reducing the aromatic sulphates. Another observation which must be admitted in evidence against the view that calomel is an intestinal antiseptic, is that of Fürbringer, who found that the stools of typhoid underwent no reduction in the number of putrefactive bacteria while calomel was being administered.

We thus have conflicting evidence regarding the influence of calomel upon the aromatic sulphates. The probability seems to be that small doses of calomel have no effect upon the sulphates, while doses large enough to

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with bromides), and in several cases of arthritis deformans (a condition which seems closely related to intestinal putrefaction). The only drawbacks to the use of the drug in these doses have been diarrhœa occasionally and eczema rarely.

cause free watery movements diminish or abolish them. But the latter fact does not require us to assume that calomel is, properly speaking, an intestinal antiseptic. Its effect upon the sulphates doubtless depends upon its cathartic properties. Food which has been lying putrescent in the intestine is hurried through with such rapidity, and such a derivation of water from the surrounding parts, that there is little or no opportunity for the absorption of aromatic substances from the intestine. This view of the case receives some confirmation from the observation of Pouchet, who found the ethereal sulphates to be very greatly reduced in the stage of collapse in cholera. The observations already mentioned, regarding the effect of croton-oil, also fall in line with this idea. While, therefore, we certainly cannot look upon calomel as an intestinal antiseptic, we must be prepared to admit that it is capable, under some circumstances, at least, of reducing the absorption of aromatic bodies from the intestine. It is not improbable that much of the good which calomel is recognized to do in intestinal disorders generally depends upon this interference with the absorption of aromatic and other putrefactive substances. To be sure, we know that benefit is obtained by the use of small doses (one grain, or less), and we do not know that such doses reduce the sulphates, such evidence as we have being, indeed, opposed to this view. The observations we now have on the influence of calomel in reducing the ethereal sulphates, are so fragmentary that we cannot lay much stress upon them; they merely suggest an explanation of the beneficial effects of calomel, and we must wait for further work to confirm or disprove this suggestion. Yet the fact remains that, in many cases where there is no apparent constipation, but the ethereal sulphates are too abundant and indican is present in large amounts, a moderate catharsis from time to time (once in five or ten days), from calomel, has an unquestionable influence in the relief of the varied symp-

toms of intestinal indigestion. In some cases of epilepsy, seizures may be, at least, deferred by this means. These effects of calomel are, doubtless, shared by other cathartics. Our own reliance has, however, been placed chiefly in calomel in small doses. In many cases of duodenal indigestion, especially when there was epigastric tenderness, the use of salts did harm, as shown by aggravation of local tenderness and pain. In other cases of intestinal indigestion, the salts currently employed (containing sodium sulphate) have given rise to a bitter, or other unpleasant taste in the mouth (suggesting regurgitation), lasting a day or more.

We come now to the treatment of excessive intestinal putrefaction by hygienic measures, especially diet. In order to approach this subject intelligently, it is essential to bear in mind the influence of diet upon intestinal putrefaction in normal persons. We may suppose that we are studying this influence in a normal man weighing in the neighborhood of a hundred and fifty pounds. A man of this weight excretes from twenty to thirty grammes of urea daily—let us say, for convenience, that this excretion averages twenty-five grammes daily. We find that, on a mixed diet, consisting of nitrogenous food (both animal and vegetable), carbohydrates, and fats, the quantity of the total sulphates bears a relation to the urea of 1 to 10, or 1 to 15, while the aromatic sulphates, in their turn, bear a ratio to the preformed sulphates of 1 to 10 to 1 to 15. Let us now suppose that the character of the diet is altered by excluding from the mixed diet all nitrogenous food of vegetable origin, and by limiting the animal nitrogenous food to meat and milk. Let us assume, further, that the amount of proteid food eaten and assimilated is such as to keep the excretion of urea about where it was before—that is, about twenty-five grammes daily. We know from our experiments that, under these conditions, the ratio of the total sulphates to the urea would remain about where it

was, but the ethereal sulphates, instead of maintaining a ratio of 1 to 10 to 1 to 15 to the preformed sulphates, would undergo a distinct diminution, so that the ratio would be changed to 1 to 15 to 1 to 25. If, instead of substituting an animal nitrogenous diet in the case proposed above, we had shut out of the diet all animal food and given all nitrogen in the form of vegetable food—that is, with the free use of the more nitrogenous cereals, as oatmeal, wheaten grits, and peas and beans,—we should have obtained a very different result.

With the quantity injested so fixed as to give an excretion of about twenty-five grammes of urea daily, the ratio of the total sulphates would have remained about the same, while the aromatic sulphates would have increased to such an extent as to give us a ratio to the preformed of 1 to 5 to 1 to 10. As already pointed out at the beginning of this article, the total sulphates are no indication of the degree of putrefaction in the intestine, but represent the sulphates derived from nitrogenous food in process of metabolism, just as urea represents the nitrogen of proteid food in process of metabolism.

The total sulphates, consequently, vary with the activity of nitrogenous metabolism, and when they are reduced by a change in the diet from animal to vegetable nitrogen, the ethereal sulphates may be in no degree decreased.

But we can not conclude from this that putrefaction has not been increased, because, had it not been, the ethereal sulphates would have been decreased in proportion to the total and preformed sulphates.

We are inclined to think that this difference in the readiness with which certain kinds of animal and vegetable proteid food undergo putrefaction in the intestine, is intimately related to the digestibility of these foods and the readiness with which they enter into the nitrogenous metabolism of the body.

Animal foods, like milk and rare beef, are readily

digested, but peas, beans, oatmeal, etc., require longer for their complete digestion, and have an opportunity to be a longer time in the intestine while the digestive process is going on, and probably more nitrogen is passed in the fæces, in proportion to the amount digested, than in the case of animal food, though of this we have no experimental proof.

In one case in which a normal man, on a mixed diet, was changed to a diet from which animal nitrogen was excluded and in which the free use of peas and beans was substituted, the amount of urea excreted was at once much diminished, although the appetite at first remained good, and there was no limit imposed on the amount of vegetable nitrogenous food taken.

Since we are able to thus regulate in some degree the extent of putrefaction in the intestine in normal individuals by means of diet, it occurs to one that excessive putrefaction may be modified by the same means. That excessive intestinal putrefaction may really be modified by means of diet, we have been able to satisfy ourselves by ample clinical and chemical evidence obtained in the study of the conditions which have been already discussed. It is probably true that the ethereal sulphates can be reduced considerably by means of diet whenever they are in excess. The surest way of accomplishing this result is to put the patient upon a diet of which milk is the chief or exclusive constituent. In a person whose sulphates are running normally on a mixed diet, such a change of diet brings the ratio down to about 1 to 20, partly by an actual reduction in the ethereal sulphates, partly by an increase in the preformed sulphates. In a person whose aromatic sulphates are in excess on a mixed diet, a similar reduction is brought about by a milk diet, but the reduction is not so great as in a normal individual, the degree of reduction being apparently proportioned to the degree of original excess. An exclusive milk diet, however, has its draw-

backs, especially if it requires to be maintained for a considerable length of time, and it is generally wise to allow some carbohydrate food and meat, while a large part of the nitrogen taken into the body continues to be in the form of milk. The only indication for an exclusively milk diet is when the sulphates are enormously in excess, and this excess is associated with symptoms which yield only when the sulphates are decreased by means of such a diet, and such cases are very uncommon. It appears to us that the principle which should underlie the treatment of all cases of excessive intestinal putrefaction is the idea of compromise; we should aim to allow the patient as varied a diet as is consistent with the avoidance of intestinal symptoms, avoiding loss of weight and inadequate alimentation on the one hand, and evidences of auto-intoxication on the other. As a rule, we may be satisfied to keep the ratio between 1 to 8 and 1 to 12; but in one case, headache and pains in the muscles made their appearance whenever the diet was so modified as to give a ratio lower than 1 to 18.

In every case where we wish to reduce the aromatic sulphates, we believe it important to exclude from the diet peas and beans (excepting, perhaps, string beans in moderation), oatmeal, wheaten grits, pearled wheat, Indian meal, etc. We are convinced by both experimental and clinical evidence of the harm which these foods do in cases of intestinal indigestion. If it is desirable to allow some cereal, there is often no objection to white hominy, farina, or rice thoroughly cooked.

Where the ethereal sulphates are in excess it is difficult to say without trial how eggs will be borne. Without wishing to make a positive statement on this subject, we believe that eggs are usually well borne, provided they are very soft, and badly borne if they are hard. From two to four eggs, boiled one minute, can generally be disposed of in the day by adults with intestinal dyspepsia, but there are exceptions to this rule, when the yolks, perhaps owing in

part to the sulphur they contain, are responsible for headache, etc. We recall one instance in which from four to six soft-boiled eggs were taken daily by a dyspeptic, who could not take one hard-boiled egg without both local and general symptoms.

Meat, especially rare beef, is one of the foods best adapted to the treatment of cases of intestinal dyspepsia. In cases of acute intestinal dyspepsia a rigid meat diet (lamb chops, beef) persisted in for a few days, brings rapid and sure relief from the symptoms. Game is to be avoided in such cases. In the majority of cases of intestinal indigestion with excessive putrefaction, the free use of butter and fats generally seems to do no harm, but there are acute cases where fats are only imperfectly digested.

The use of kumyss in cases where intestinal putrefaction is excessive has been recommended by Rovighi, Pöhl, and others. Rovighi found that by taking daily a liter and a half of kumyss, he was able in a week's time to reduce his ethereal sulphates from 1 to 10.7 to 1 to 20.3, and he concludes from this experiment that kumyss has a pronounced influence as an intestinal antiseptic. This he attributes chiefly to the lactic acid which it contains; but he admits that this only partially explains the results obtained with kumyss, for he found that large amounts of lactic acid had much less influence than the kumyss. We are prepared to admit the value of kumyss in cases where intestinal putrefaction is excessive, but it is not yet clear that it is superior to milk as a poor medium for the growth of putrefactive bacteria in the intestine. Rovighi found that the small amount of indican which his urine contained disappeared when he took kumyss.

We have spoken heretofore only of the influence of diet upon the aromatic sulphates as a group, without reference to the influence exerted upon the particular members of the group. As yet nothing is known of the relation of diet to the excretion of phenol-potassium sulphate and skatoxyl-

potassium sulphate. The influence upon indoxyl-potassium sulphate, also, is but imperfectly understood. The general statement may, however, be made, that the dietetic conditions which reduce aromatic sulphates as a whole, reduce the quantity of indigo blue in the urine. But to this general statement there are important exceptions. In almost every case where indican is habitually present in large amounts, it has been found that the sulphates were far more readily influenced by a change of diet than the indican, which may continue for a time, at least, almost unaffected.

In the cases of intestinal indigestion, on the other hand, where there is present only a moderate amount of indican, and this is associated with and due to the same conditions that cause flatulence, the indigo blue disappears promptly under dietetic treatment. We suspect that the cases which yield so slowly, or not at all to treatment, are generally those in which there is at some point in the small intestine, probably in the duodenum, a process of chronic catarrhal inflammation.

In the treatment of any case of excessive intestinal putrefaction, the evil effects of physical overfatigue upon the course of intestinal digestion should be borne in mind. The influence of sexual excitement must also be remembered, and if there is evidence that digestion is hindered by the sexual act, this should be so far as possible discouraged. This applies especially, though not exclusively, to males. The possible influence of masturbation in causing intestinal indigestion must always be remembered.

Where excessive intestinal putrefaction is associated with an excessive output of uric acid this condition should receive special attention, the influence of carbohydrates in the production of the excess being borne in mind.

Regarding the treatment of epilepsy in those cases where intestinal putrefaction is in marked excess, it must be freely owned that the results have not been encouraging.

In a great many cases there has been brought about some improvement of a temporary character, by means of treatment addressed to the intestinal condition.[1] In fact, it may be said that some improvement has usually been observed in the number of *grand mal* seizures when an intestinal condition has been much improved. There are, however, many epileptics in whom the intestinal condition is so pronounced and of such long standing that we can do little to improve it. In such cases we can not, of course, expect improvement in the seizures. Then we must not lose sight of the element of cerebral instability which is brought about by the frequent, long-continued repetition of the cortical discharges. In adults with an established epileptic habit we can not reasonably hope to do more, through the treatment of the intestinal trouble, than to remove or diminish one of the conditions that influence the frequency of the epileptic discharge. In some instances this means material improvement.

In some cases of epilepsy in children, we may reasonably hope to accomplish more. We have met with cases where, after a prolonged period of dietetic indiscretion, a child has had for the first time a series of epileptiform convulsions, which have disappeared for many months (the patients being finally lost sight of) after a careful correction of the dietetic error. It is likely that in children epileptiform seizures recur and become chronic through the

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[1] The following notes upon trials of intestinal antiseptics in epilepsy were furnished us by Dr. Frederick Peterson:

In one case a patient had under bromide treatment for two or three years an average of forty-five attacks of *grand mal* a month. Put upon salicylate of sodium only, the bromides being dropped, the fits were reduced in number at once to twelve a month. In three other cases similarly treated, either with salicylate of sodium or salol, a reduction of one half was made in the monthly number of seizures. These were cases that had been kept under long observation and were selected from sixty cases in Peterson's charge at the New York Hospital for Nervous Diseases. In two of his private cases improvement of like degree was observed upon following out this line of treatment. It is but fair to say that in half a dozen others no such good effects were obtained.

non-correction of intestinal states, the element of cerebral instability being at first slight, but tending with each repetition of the epileptic discharge to the establishment of a habit. It can not be said that we have proof that this is so, for, from the nature of the case, it is difficult to say what would constitute proof, but it is sufficiently probable to make it worth while to pay particular attention to intestinal conditions in all recent cases of epilepsy in children in which careful examination and the history of the case render the existence of an organic cerebral process unlikely.

In conclusion, it may be said that while much remains to be learned, especially regarding the relation of the various individual putrefactive substances formed in the intestine to the symptoms of disease, enough evidence has been accumulated to show that excessive intestinal putrefaction is a not infrequent occurrence and is in some way related to the production of symptoms. Hence it constitutes a pathological condition whose recognition gives us at times distinct indications for treatment, which it may render both more intelligent and more effective. It remains for future clinical observation to establish the range of utility and the limitations of measures directed to the correction of this condition.

NOTE.—The methods of chemical analysis employed to obtain the results reported in this paper are:

Urea, estimated from the amount of nitrogen determined by the Kjeldahl method.

Uric acid, after the method of Salkowski, as modified by Ludwig and further modified by Groves.

Total sulphates, by weighing as $BaSO_4$ after the usual manner.

Combined sulphates, gravimetrically, according to the manner described by Baumann and modified by Salkowski.

Indican, method of Jaffé, oxidizing with calcium hypochlorite and separating by shaking with chloroform.

DISCUSSION.

DR. LANDON CARTER GRAY.—I have been very much interested in this paper of Dr. Herter's because it is one of the few original papers that I have ever had the pleasure of listening to. The subject is such a vast one, and includes so many features which are still very much obscured, that it possesses all the more interest to one who sees, as I do, cases of nervous disorder which cannot be classed under one particular head, and in which the urine contains the products of intestinal putrefaction. Time will not permit me to give the details of certain observations made by myself upon the urine during the past few years, bearing on this subject; these I hope to lay before the Academy in a paper, at no distant date. I would simply like to say, that in almost every instance my results would bring me to the same conclusions as Dr. Herter's have brought him. I also agree with what he says about the inadequacy of the means at our command to correct intestinal putrefaction, although I would not be quite as positive in my conclusion on this subject as he is. In some cases I have found that salol gives marked relief. In other cases I have found that a combination of salol, bicarbonate of soda, and pancreatin would afford marked relief where everything else failed; after a few days, this combination can be followed by the use of salol alone. Again, in certain cases of supposed intestinal putrefaction which lead to such a chronic disturbance of the entire system—such cases as a few years ago would have rendered possible the diagnosis of malaria—antiseptics and laxatives will do no good until a few large doses of calomel have been administered; after this has acted the combination of salol, soda bicarbonate, and pancreatin will often prove very beneficial. In some cases I have seen remarkable results follow the use of subgallate of bismuth, which was recently recommended by Dr. Austin Flint, while in other cases it totally fails. Taking it all in all, in cases where there has been reason to make a diagnosis of intestinal putrefaction, the patients were more successfully relieved by a combination of disinfection with a mild laxative than by disinfection alone. As regards the selection of a proper laxative, I have found that the best one is the ordinary Rochelle salt, given every day or every second day; this does not produce depres-

sion, and in some of these cases with complex symptoms, it will, in the course of two or three weeks, produce marked improvement in the patient's general condition.

In listening to Dr. Herter's paper, I was rather sorry to see that he did not take into consideration the question of the amount of uric acid in the urine in connection with intestinal disinfection and putrefaction. I do not say this in a spirit of criticism, but because I was desirous of getting his views for my own information. I have recently read a very charming book written by an English physician, Dr. Haig, which makes you think that the whole problem of the practice of medicine has been solved, and which really does carry you through many obscure gaps, and in which he makes some interesting statements about the connection between uric acid and intestinal putrefaction. During the past eighteen months, in all my cases of organic and functional nervous disease, I have carefully examined the urine, and in almost all cases I have found that when the patient was put on an exclusively nitrogenous diet the uric acid increased, and, according to the disease, certain symptoms became aggravated; whereas, if the patient were put on a vegetable diet, the amount of indican in the urine increased.

DR. CHARLES L. DANA.—I have been extremely interested in the subject brought before us by Dr. Herter, and feel as Dr. Gray does, that we are very much indebted to Dr. Herter for pushing his inquiries along a line which involves so much careful and diligent research. He has already written one or two papers on this subject, and neurologists are profiting by his investigations. In the cases that have come under my care during the past two years, I have borne in mind the influence of uric acid excess and intestinal putrefaction. I will therefore limit my remarks to the results of my practical experience in the treatment of these conditions. I will say in the first place, that I have adopted almost as a routine matter, the test which Dr. Herter has popularized for the detection of indigo blue in the urine, and have found it very satisfactory, because it tells us the amount of uric acid, or rather whether there is an excess of uric acid present, and also gives us information as to the presence or absence of indican. I have obtained practically the same results as he has in connection with intesti-

nal disinfection. An excess of indigo blue and uric acid is often present in epilepsy and neurasthenia occurring in adult life. It does not occur in the adolescent types of neurasthenia, those which we might call of primary character. In attempting to treat them, I have met with the same difficulties and varied results that have been the experience of Drs. Herter and Gray. Sometimes, under the influence of a corrected diet, or some antiseptic remedy which tends to clear up the intestinal disturbance, an improvement has taken place, but in the majority of cases I must confess that I have been rather disappointed, and I do not now look for an absolute cure by the simple treatment of these conditions. I remember a number of years ago it was the fashion to think that neurasthenia was due to the presence of an excessive amount of uric acid; and at that time, largely under the teachings of Dr. McBride, we heard much of lithaemia; then we were told it was due to eye strain; now we are turning our attention to intestinal putrefaction, and I think that out of each one of these experiences we are deriving some good, but we have not yet found any single key to the solution of this problem.

As regards the practical measures that are employed in the treatment of this condition, I think we must bear in mind this fact; we must secure not only a certain amount of intestinal antisepsis, but must also promote the action of the liver and emunctories, and I have found, as Dr. Gray has, that a combination of intestinal antiseptics with a mild laxative produces much better results than the antiseptic alone. I have had the best and most striking results from the use of naphthalin. This drug, in doses of from sixty to eighty grains daily, has been recommended by an English alienist for insomnia in certain classes of insane. I have used it in a number of cases of neurasthenia, and, in this class of cases, as well as in migraine, it sometimes acts like magic. In one case I remember its good effects particularly impressed me; the patient was a man who for three or four years had suffered from neurasthenic headaches. He had his eyes examined and various refractive errors corrected, but his headaches persisted. Moderate doses of naphthalin were then administered, and his pain was entirely relieved.

Dr. William H. Thomson.—I have long thought that

the subject of functional nervous diseases would have more light thrown upon it by a study of chemistry than by that of pathological anatomy. The striking feature of intermittency pertaining to functional nervous diseases, such as epilepsy and migraine, is certainly worthy of attention, and is in striking contrast with that observed in organic nervous troubles. I have therefore always felt a very great interest in the question of auto-infection from the alimentary canal, and for years it has been my practice to investigate very closely the condition of the intestinal functions in epilepsy and other functional nervous disorders. One of the first questions I put to a patient who gives a history of epilepsy is whether preceding or during or immediately subsequent to an attack the breath is particularly offensive. In about thirty per cent. of the cases that have come under my care, an affirmative answer is given to this question. In a still larger percentage of cases (about 40%) the patients give a history of intestinal disturbance, such as flatulency, etc.; in a small percentage of cases the attacks were preceded or followed by diarrhœa. In these cases my treatment has been mainly directed towards a modification or correction of the intestinal disturbance, and thus far my results have been very encouraging. The questions of diet and intestinal antisepsis are very difficult ones to settle, but, by the light of clinical observation I think we are on the right track, and what will aid us still further in finding and keeping the right track are investigations of the character Dr. Herter has brought before us to-night. Clinical observation has so many factors of uncertainty that we cannot get positive information from it. We must look to the chemist to tell us, by an examination of the excretions, what changes have taken place in the alimentary canal.

My experience with intestinal antiseptics has been very much in accord with that of the previous speakers. Salol I have employed extensively, but when given in sufficiently large doses to prove efficient it frequently produces so much gastric disturbance that I now always give it in moderate doses and in combination with other drugs. Benzoate of soda is a favorite prescription of mine in the treatment of these cases. I would like to ask Dr. Herter whether he has used it; also whether he has used bismuth

in large doses. In typhoid fever I give from eighty to one hundred and twenty grains of bismuth daily, and I think it has considerable effect in limiting the putrefactive changes in the intestines in that disease; so much so, that in my hospital ward it is very rare indeed for any of the patients suffering from the disease to have diarrhœa. At the Roosevelt Hospital during October—which is typhoid fever month—there was only one case in which the diarrhœa lasted for more than two days.

In epilepsy, with a history of intestinal disturbance, I prefer the use of benzoate of soda and bismuth, with moderate doses of salol. I can confirm what Dr. Gray said about the calomel purge. I also employ calomel to check the diarrhœa in typhoid fever; it very rarely fails. I also give it in scarlet fever and other exanthemata. Whether it acts as an intestinal antiseptic or not I do not know, but it certainly checks diarrhœa when given in a purgative dose.

With reference to diet, it has been my practice for years in the treatment of epilepsy to cut off nearly all flesh food, relying on milk for the nitrogenous element, especially in the form of matzoon or fermented milk, and a moderate amount of vegetable food. Certainly a vegetable diet may at times be harmful to persons who suffer from an excess of intestinal putrefaction. I have been struck with the immediate increase in the amount of albumin in the urine in cases of chronic Bright's disease after allowing the patients to take either peas or beans. I have a series of observations recorded on that point. Beans particularly are more likely to aggravate the renal trouble in cases of chronic Bright's, especially contracted kidney. Probably beans increased intestinal putrefaction, aggravated kidney trouble, and enhanced the dangers of convulsive nervous disorders. There is a very great difference between nitrogenous food, as found in milk, and as found in flesh. The fact that almost all carnivorous animals, particularly cats, are subject to convulsions, gives some reason for believing that a meat diet is not favorable for epileptics. The fact that in some of my cases the epilepsy returned on the patient's resuming a meat diet, has helped convince me that it is important to cut off the flesh food in these cases.

DR. L. EMMETT HOLT.—There is one class of cases of which I have seen quite a number that exhibit in a striking degree the effects of intestinal putrefaction. I refer to children between the ages of one and four years. They are usually children with very spare bodies and immense abdomens, many of them more or less rachitic and all suffering from chronic constipation. Usually you get the history that their food has been composed almost exclusively of carbohydrates, particularly potato and oatmeal. The treatment almost uniformly followed has been to take away most of the starchy food and put the patients at once upon peptonized milk and raw or rare beef. As regards diet, children are much more easily managed than adults. Investigation has shown, contrary to the general supposition, that in some cases the main source of the trouble in these cases was the caseine in the milk; as soon as peptonized milk was given, immediate improvement in the child's condition took place. In addition to the peptonized milk, these children are given large amounts of very rare, red beef; the steak is broiled for a few minutes and then scraped. A dose of calomel is given every three or four days to relieve constipation, although that is pretty well controlled by the diet if the milk is well peptonized and contains a little more than four per cent. of fat. This method of treatment has answered so well that drugs have been almost unnecessary; their general nutrition has improved, there is a gain in weight and a diminution in the size of the abdomen and in the amount of flatulence.

DR. HERTER (closing the discussion.)—I have very little to add to the discussion. In regard to salol, I think it acts as an intestinal antiseptic, but I could not determine this by the study of the sulphates, on account of the fact that the drug is eliminated in the urine as an ethereal sulphate. I have employed Rochelle salts, in some cases with good effect, in others with bad effect. I have known cases in which the administration of the salts gave rise to a good deal of pain and irritation, apparently in the small intestine. I never give them in cases where there is much epigastric pain or tenderness. Kumyss, as regards the elimination of ethereal sulphates, ranks about the same as milk. In cases where the stomach is upset I think it

acts better than in intestinal cases. How it acts differently from milk, I do not know. I think that the use of meat—especially rare meat—is one of the most valuable means at our command for the control of intestinal putrefaction, and even in epilepsy I recommend it in small quantities. I think the use of large quantities is detrimental. With meat you can reduce the quantity and still get just as much good nitrogenous food into the body with small doses as you can with almost any other form of nitrogenous food in large doses, except milk. I was much interested in what Dr. Holt said regarding the use of peptonized milk and rare beef in the treatment of intestinal putrefaction in children. It helps bear out my ideas on this subject.

Stated Meeting, February 1st, 1894.

THE PREVENTION OF DISEASE.

A PROBLEM FOR ALL PHYSICIANS.

By WILLIAM WARREN POTTER, M. D.,

BUFFALO, N. Y.

The physician exercises his highest function in the prevention of disease. This is neither new nor original, but is, nevertheless, as true as when it was first uttered in reference to modern physicians and modern medicine.

In a medical society meeting, it is far more entertaining, and even fascinating, to describe a brilliant clinical success, either in surgery or medicine, than it is to discourse upon the methods of prevention, as applied to the maladies of mankind. But it would be unseemly to address this Academy, in general session assembled—composed, as it is, of men who are distinguished in all the specialties of their art,—upon any one branch thereof, expecting to attract either the ear or the thought of anything like a considerable number, to say nothing of a majority. But every one is, or ought to be, interested in the prevention of disease. Moreover, the time has fully come for the discussion, in all medical sociëties, by all medical men, of the manifold questions relating to preventive medicine.

The problems that are involved in this department of medicine must interest the general practitioner, whether of medicine or surgery; likewise all specialists in the several departments and branches of medical science and art; nor can the laboratory teacher, whether he be chemist, pathologist, physiologist, or bacteriologist, escape the necessity of a knowledge of how to prevent the diseases

whose infection and contagion he is absorbed in studying and analyzing. What man is there who bears the honorable title of "Doctor of Medicine," no matter what his specialty in teaching or practice, who escapes the sometimes intelligent and always persistent interrogations of his patients or friends with reference to the best methods of preventing the spread of cholera, small-pox, diphtheria, typhoid fever, and the like? He must be prepared to offer an intelligent opinion on all such questions, as well as on those pertaining to plumbing, sewerage, drainage, ventilation, and every detail of house sanitation. Ignorance on, or indifferent acquaintance with, these subjects is a confession of unfitness to retain the title. Happily, however, some of the exanthematous fevers have not now the same dread to humanity as formerly, since the triumph of medicine in solving the methods of their prevention and cure. But if this be true in a certain degree, there is being added to the list a number of maladies that formerly were not thought to be contagious, or, at least, infectious, but have now become well known to be so through the investigations and studies of clinicians and men experienced in the science of bacteriology.

Permit me to illustrate by accentuating the importance of a better knowledge concerning the prevention of the propagation of a few of the more serious diseases of this class.

I may refer, first, to one disease—namely, *consumption*—that comparatively lately has become known to the profession as an infectious disorder; a malady that is destroying more inhabitants of the globe than almost any other known disease, and that last year proved fatal to more than six thousand residents of your own city.[1]

While it is true that much has been done of late to control its spread and to abridge its ravages in localities, yet

[1] Report of Dr. Hermann M. Biggs to the Health Department of New York City.

how many physicians are ready to admit even now that it is an infectious malady, and that it may be conveyed to a healthy person who is habitually in the atmosphere of a consumptive? If it were possible to apply the knowledge which a few of the more advanced—a small minority—of our profession now possess with reference to the propagation of consumption, and to disseminate this knowledge among the mass of physicians, it would be serving humanity in the highest possible way, concentrating its efforts toward preventing the spread of this direful disease. In order to do this effectively, it would become necessary at the outset to establish an absolute inspection of all foods of an animal nature, as well as the supervision of the care of all animals that furnish food for mankind, with a view to rigid condemnation and destruction of such as prove unfit.

Again, if we should undertake to prevent the spread of consumption by the application of all the intelligent means at our command, it would further involve imperious supervision over the food of our cows, and their stable care, as well as over the animals that are slaughtered for our daily consumption. It further also would involve the inspection, by a competent veterinary surgeon, of all cows that afford milk supply to our cities and villages, to the end that every tuberculous beast should be killed and its body cremated. I consider it essential that the latter order shall be enforced with great exactitude. If any portion of a tuberculous animal, excepting its hide, horns, and hoofs, be permitted to enter the avenues of commerce, there is danger of the propagation of disease, and I know of no way to effectively abolish such danger, excepting to imperiously insist upon the incineration of all tuberculous carcasses.

But we must not stop here; our food supervision must extend to all domestic birds, fowls, and animals whose milk or flesh we use for food. The quantity of tuberculous food consumed by the people assuredly is enormous,

and it will require the most rigid system of inspection and supervision by most skilled, honest, and well-paid officials to interrupt, cut short, or prevent traffic in these dangerous and unwholesome ailments.

Besides this, the circumspection and control of railway and steamboat travel, as well as hotel life, would become necessary, and this would simply arouse an opposition from every traffic official and landlord in the country, to say nothing of the indignation of travelers themselves. Yet none such would complain at the exercise of an absolute authority with reference to restraining the transit of small-pox patients, or those affected with diphtheria or cholera. Nevertheless, these maladies, one and all, have not at present one scintilla of the importance connected with their destructive powers that has consumption.

Another most important step toward the prevention of the propagation of consumption consists in the prohibition of the intermarriage of tuberculous subjects. If a man has tubercular disease, he should not be allowed to marry a healthy woman, and thus subject her to the danger of contracting the malady from cohabitation with her consort, to say nothing of the liability of propagating tuberculous offspring. For like reasons, a tuberculous woman should be prohibited from marrying a healthy man. But if there are cogent reasons why these should not intermarry, there are still more forceful arguments that may be offered against the intermarriage of two tuberculous individuals—that is to say, a man and a woman who either have tuberculous disease or who have come from tuberculous families. I make this assertion at random, without desiring at this time to enter into a discussion as to whether the laws of heredity govern in consumption or not. But of this I am certain, that for a man with existing tubercular disease to marry a woman of tubercular parentage would be tempting fate with a rude audacity deserving a better cause. The offspring of such a mar-

riage, to say the least, would offer a favorable soil for the growth of the tubercle bacillus. I earnestly hope that before long an enlightened legislature will enact statutes that will prohibit such marriages.

Still another step in the direction of prevention consists in the establishment of separate hospitals for consumptives, and the adoption of measures looking toward their isolation. But here most bitter opposition will be encountered from those who are governed by sympathy, affection, and kinship. Nevertheless, it were well-nigh impossible to prevent the spread of this fateful malady unless the methods and habits of life of its victims can be assuredly controlled. It is admitted by those most familiar with the methods of the propagation of the disease, that the sputum of the consumptive constitutes a focus of special danger. Hence, to deal intelligently with the question of prevention, the sputum must be sterilized as soon as it is thrown off. Nor will it answer to deal with it in a half-hearted, indifferent, or slipshod manner; the sterilization must be absolute and immediate, and this can only be accomplished under the eye of a trained expert who knows no law but that of perfect cleanliness and absolute sterilization, and who will not be diverted from the end in view by fear, favor, or affection. I know of no way to effectively enforce rules to prevent the propagation of consumption, except to remove tuberculous persons from all possibility of contact, direct and indirect, with the healthy community.

Without doubt, independently of the infection by food, the most serious menace comes from the respiration of infected dust, and it needs no specious ratiocination to prove to such an audience as this that the one great means of infecting dust is through the sputum of consumptive patients. It has recently been said by a writer in the *British Medical Journal* that it would be well for the medical profession at large to recognize the infectiousness of tuberculous sputum and the danger of letting it dry into

dust. Marpman (*Centralblatt für Bacteriologie und Parasitenkunde*, Band xiv, 8, pp. 228–234; *Med. Chron.*, November, 1893) examined the dust of a frequented street in Leipsic, and found the bacillary remains on about eighty per cent. of samples of dust examined. From the researches of Marpman, we are taught that street dust may easily prove a source of infection, and that people should be taught to desist from expectorating in thoroughfares or public places. Houses that have been occupied by consumptives, too, should receive the most thorough renovation before further use.

It cannot be denied that already much has been done to limit the spread of tuberculosis, as a careful examination of statistics would prove, but these we have no time here and now to enter into. The results already accomplished have been outside of the prevention of intermarriage, inspection of food, attempt at isolation, or sterilization of sputum, and have been mainly achieved through the adoption of improved sanitary regulations—better nutrition, purer air, and other influences of like character.

I stand here to plead for the enforcement, in addition to these well-known sanitary measures, of the prevention of intermarriage, of the rigid inspection of animals that furnish food, and the isolation of tuberculous patients, and I ask this Academy to stand together as one man until it has brought about such a change in public sentiment and improved statutory law as shall make all these conditions possible. I believe it will be yet just as possible to quarantine and control the methods and habits of life of the consumptive as it is now those of a small-pox, yellow-fever, or cholera patient. At all events, I certainly hope that an enlightenment of the people on this subject will lead to such results.

Turning from the consideration of tubercular disease, we approach another infectious malady—*typhoid fever*—which has attracted the attention of physicians and sani-

tarians for more than a generation. In July, 1845, Dr. Austin Flint, Sr., then of Buffalo, and afterward one of your distinguished academicians, published in the *American Journal of Medical Sciences* an account of an epidemic fever which occurred at North Boston, Erie county, N. Y., during the months of October and November, 1843.

It appears that in September, 1843, a young man from Massachusetts stopped at Fuller's tavern in North Boston. He had been unwell for several days, a diarrhœa being among the most prominent of his early symptoms; he was first seen by a physician only six days before his death, who found him suffering from diarrhœa, low, muttering delirium, sordes, and other symptoms, that, grouped, we nowadays characterize as a typhoid condition. He lived twenty-eight days, and from this case the fever spread to other members of Mr. Fuller's family and to those living nearest to his house. Seven of Mr. Fuller's family had the fever, of whom three died. Twenty-one other cases occurred in five families, all being within ten rods of Mr. Fuller's house, of whom seven died, making in all, including the stranger, twenty-eight cases and ten deaths. The whole population of the hamlet amounted to forty-three persons; none were attacked who were over twenty-three years old; the youngest was only a year old, and a large proportion were children.

Dr. Flint visited the locality of the epidemic, during which time he made a post-mortem examination of the body of a child about twelve years old, and took notes at the bedside of the histories of nine cases, together with symptoms presented at the time of his visit. It was his first distinguished success as an investigator. He found and diagnosticated a fatal disease then unknown in western New York, traced it from New England to the hamlet of North Boston, distinctly established its contagion, and found its focus in a particular well of water which had been poisoned by the excreta of an original case. Further

use of this well was prohibited. The cases then existing recovered, and no new ones appeared. The work was done with all the precision and completeness that characterize the very best detective work, coupled with all the skill and acumen of the scientific clinician. Nothing was omitted that would contribute to the completeness and convincing power of the evidence adduced, and the published report has become a classic in this country and in Europe. It also formed a basis of a series of essays that Dr. Flint subsequently wrote on typhoid fever.

Thus was established for the first time a direct relationship between poisoned excreta and the contagion of typhoid fever. I wish we also could say that there had been established an absolute knowledge of the contagium of this destructive malady and the methods of its implantation. Who can say to-day with absolute certainty that the infection of typhoid can be prevented in a given case? Who has yet been able to solve the subtlety with which the bacillus of typhoid fastens itself upon the organism, or who can say what is the most favorable soil for its implantation? Medical societies are constantly debating these questions, and the pages of medical journals are teeming with learned expositions of the various and manifold minutiæ of the subject. I listen to these debates, and am, to a certain extent, familiar with journalistic literature. Yet I fail to find a satisfactory answer to the various questions bearing on the communicability of typhoid, or any final and determined method of preventing its propagation. There are nearly as many opinions on these several details as there are participants in the debates or writers in periodicals. All this indicates that there is much yet to be done in experimental research, clinical observation, and bacteriological study in relation to typhoid fever. If a conclusion is ever to be reached thereon that will be accepted as final by the profession, it must be through the combined efforts of clinicians, pathologists, bacteriologists,

and sanitarians. In the city of Chicago alone, from January 1, 1890, to November 1, 1893, there have been, according to published reports,[1] five thousand and eighty-seven deaths from typhoid fever—an average of a hundred and ten a month. This indicates that during this period there have been between thirty and forty thousand cases. There are, says Vaughan, annually fifty thousand deaths in this country from typhoid fever, and half a million sick with it.[2] If it were possible to estimate the money lost to the community in salaries, wages, and expenses from this one disease, the sum would be something enormous to contemplate; and yet there is a great outcry in most cities against the ordinary expenses of the health department. This embarrasses the officials. The evil should be corrected through an enlightenment of the masses on the subject. If their sympathies cannot be reached from the humanitarian side of the question, then their interests must be appealed to from its economic aspects.

Similar statements may be offered with reference to *diphtheria*. Who can say what is the precise method of the propagation of this subtle poison? Who has yet ventured to tell us absolutely how it enters the system, what is its favorite soil, and why, under apparently like conditions, one individual contracts the disease, while another has only a mild malaise, and still another goes scot-free? Who has told us with absolute certainty what part mephitic vapors play in the causation of this disease? In nearly all the large cities of the world, during every month of the year, cases of diphtheria are reported, ranging all the way from a few scattering victims to many hundreds and even thousands, with a fearful percentage of mortality. Here is another field where moot points are many, opinions widely differ, and satisfactory conclusions are still in the air. Shall it be said during all the future years that diph-

[1] *Med. Record*, 1893–xliv–841.
[2] *Med. News*.

theria baffles all attempts at prevention—that it paralyzes the efforts of the medical profession to control it or to fix its habitat? This would be a sad confession of weakness, and a stigma upon the science of medicine that ought not to be permitted to remain. Let us address ourselves to the study of its prevention with an assiduity that will at least deserve success.

But one other example, yet in a most important field, will be cited at this time. I refer to *gonorrhœal infection in women.* It is doubtful if any disease is capable of propagating more of sickness, woe, and misery than a neglected or uncured gonorrhœa. In view of recent researches, and especially of clinical experiences evolved at the operating table, it is now admitted that what was taught and written relating to the treatment of this infection prior to 1872 was little better than so much chaff; and this remark applies with equally cogent force to the acute stage in the male and the chronic stage in the female. Though it is true that the medical profession gave little heed to the notes of warning sounded by Noeggerath when he published his opuscule in 1872, now, after twenty years of continual hammering, the foremost men in medicine have finally become aroused to the dangers that lurk in an uncured or latent gonorrhœa. This is especially true of genito-urinary surgeons and of those gynæcologists who practice pelvic surgery. Other specialists and clinicians who listen to their debates and read such convincing expositions of the subject as have been given, on the one hand, for example, by your distinguished academician, Brewer, and, on the other hand, by Cushing, of San Francisco, have accepted in a measure their teachings.

Brewer has conclusively demonstrated in a clinical history that after six years had elapsed since infection, and where upon examination no secretions could be pressed from the urethra, yet endoscopic examinations revealed granular patches and congested areas in the neighborhood

of the bulb and behind a stricture of large caliber measuring 33°. In this case, the microscope showed several characteristic colonies of gonococci. Six weeks afterward, contrary to advice, this man married, and two weeks later had communicated the infection to his wife, in whom it went through the various stages of inflammation and resulted in abscess.[1]

Cushing has shown that a woman who had been infected five years previously, without intermediate exposure, finally married, and infected her husband, who had a severe urethritis, which resulted in a perineal abscess. Cushing removed both tubes and ovaries, that were distended with pus, in which gonococci were found. The secretion from the urethra of the husband was examined, and gonococci demonstrated without difficulty. Here, then, are two cases: one in which the infection was carried by a man for six years and then communicated to his newly-wedded wife; and the other, in which it was carried by a woman for five years and then communicated to her husband, who had all the symptoms of acute gonorrhœa resulting in abscess.[2]

I have cited these two examples as types of this disease, showing its subtlety, how it lurks in some nook or cranny of the genital tract for years, thence to be conveyed to another person, in whom it lights up a disease that ravages the entire pelvic cavity. The list might be prolonged indefinitely, but this is neither the time nor place to recite lengthy clinical records.

Only within the last few years have physicians been ready to admit the terrible destructiveness of this malady. Until lately, most physicians or surgeons who have been called upon to treat the disease in its acute stages, whether

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[1] The Contagiousness of Chronic Urethral Discharges. *Journal of Cutaneous and Genito-Urinary Diseases*, 1891–ix–81.

[2] Contribution to the Study of Pelvic Diseases. *Transactions of the American Association of Obstetricians and Gynæcologists*, 1888–i–17.

in the male or female, have generally regarded it in a light or trifling manner, contenting themselves for the most part with ministering to the first distress, but paying little heed to its secondary probabilities. The victims of this disease, too, have been taught to regard it as a mere trifle that a few urethral or vaginal injections and a few doses of copaiba would assuredly cure without danger of any secondary complications, and that even a long-standing gleet had little or no significance beyond the mere personal discomfort that it caused. Only the few even consulted physicians, while the many borrowed a syringe and a prescription from a friend, or obtained the same from a kindly and sympathizing druggist, who, for a few shekels, is ever ready to assure the sufferer that his disease is of slight consequence, that he will be well of it in a few days, and that all he needs is a few injections with "No. 1" and a few days' dosing with "No. 2."

The literature of this disease is being almost entirely rewritten, and the teaching of the schools is changing, in view of the awful consequences of neglect to cure or prevent the propagation of this subtle, deceptive, and virulent infection when once it has been contracted. It has come to pass that a woman who accepts an offer of marriage finds herself treading upon dangerous ground,—a danger far more serious than those contemplated by Mother Eve when she entailed her sex,—and it becomes a problem of the most serious import when her answer may fetch her to a spring of woes unnumbered. I speak seriously, for it is a subject that demands our most thoughtful consideration. If we would stamp out the frightful malady, or even prevent its propagation to innocent persons, we must, as a profession, exert ourselves to the utmost to bring enlightenment to the people and to enforce an intelligent application of the laws of prevention. We have been taught to regard syphilis as the most destructive of the venereal diseases, but I believe gonorrhœa to be a far more serious disease

than syphilis, and that it plays more dreadful havoc with the community. We are seeing constantly fewer and fewer syphilitics, while gonorrhoics are increasing in number, and the secondary consequences of the disease are becoming better understood. I am well aware that difficulties surround this subject almost greater than any other, owing to its extreme delicacy; nevertheless, the fullness of time has arrived when action must be taken to arrest a disease that leaves such horrible consequences in its track, and I appeal to this Academy to formulate and carry out some plan that shall have for its purpose the enlightenment of the community, the education of physicians, and the enactment of such statutes as shall prevent others than physicians from prescribing or in any manner giving advice as to the treatment of gonorrhœa.

CONCLUSIONS.

In my early youth I learned to look to the New York Academy of Medicine as the leader of medical thought in this country. My father, a physician, so taught me, as a child receives impressions from the hornbook on his mother's knee. It is here that medical men from all quarters of the globe are received and entertained at your meetings, and go away with new inspirations, renewed energy, and increased intelligence. This Academy becomes, perforce, a mighty instrument for the advancement of medical science and improved methods in medical art. One of the great orators of this metropolis asseverated, in his speech at Chicago on Manhattan day, that the World's Fair had taught us that art was more godlike than science, for, while science discovered, art created. I believe this applies as well to medicine as to other arts and sciences. I ask you to remember that I am not unmindful of the great work this Academy has inaugurated from time to time during its history, with reference to the prevention of disease, nor of the fact that it is still laboring with might in that field. When

this Academy speaks, the world moves. When she asks that certain methods and certain means be adopted with reference to improvements in quarantine, they are done. When she recommends the board of health to adopt certain measures with reference to the details of preventive medicine, the board at once accepts its conclusions and adopts them. It has gratified me to observe very recently the splendid action taken by the Academy with reference to the creation of a National Bureau of Health, and I sincerely hope its recommendations may be given form and force in the immediate future through national statutory law.

But it is not as sanitarians, or public health officers, or as administrators of state medicine, that I appeal to this Academy of Medicine. I beseech you, each and every one, in your capacities as individual members, to do your parts toward inaugurating a fashion that shall have for its purpose discussions relating to the prevention of disease, at the general meetings of this Academy, to the exclusion of all other scientific work. The report of these proceedings given to the medical journals and published as *ex cathedra* edicts will arrest the attention of physicians all over the world, and thus serve to begin a campaign of education on the several questions to which I have called attention, as well as many others that for obvious reasons could not be specified on this occasion. I have merely attempted to accentuate those that seemed to me of greatest importance. Cholera, small-pox, and even yellow fever are no longer regarded as plagues of humanity, but consumption, diphtheria, typhoid fever, and gonorrhœa are slaying or invaliding *unnecessarily* hundreds of thousands annually.

The laity must be educated to regard consumption as a communicable disease, and to accept laws and regulations calculated to restrict its propagation and restrain its victims within the limitations of safety, so far as uninfected individuals are concerned. But rules, too, must be formulated

for the prevention of the spread of diphtheria and typhoid, and these must be insisted upon without deviation, and the community must learn to yield a hearty support to such beneficent and humane methods as have for their sole and distinct object the protection of the individual. Finally, I hope that this Academy will address itself seriously to the eradication, prevention, and cure of that loathesome, subtle, and destructive disease, gonorrhœa, that is directly and indirectly bringing to invalidism or death so many innocent thousands throughout the civilized world. I am well aware that all these questions are as well or better known to you than to me; I am only attempting to recall them to your minds for the purpose of stimulating or encouraging renewed energy in their consideration.

Pardon me, Mr. President, if I have said more than I ought to have said, or more than there was occasion to say. I have spoken from a deep conviction of the importance of the subject, and with a realizing sense that I am standing in the presence of men who are doing as much or more than any other group of equal number toward the prevention of the diseases in question, as well as others that have not been mentioned. I know, too, that your sections are made up of recognized men of exceptional skill in their several departments, and whose work goes out every month to the professional world in brilliant and well-recorded achievements. Your orthopædists are restoring hundreds of maimed children to usefulness and self-support; your ophthalmologists are restoring to sight thousands of eyes every year, and are preventing the beginning of destructive processes in many thousands more; your gynæcologists are restoring to health numberless invalid women—wives, mothers, and sisters—who would otherwise drag through life a miserable existence or obtain all too early sweet relief in death; and so on through the entire list of your splendidly organized sections. But in spite of all this, let me implore you to set apart at least one

meeting in every month, if not both, where all the splendid talent of these sections shall merge itself for the discussion of problems relating to the prevention of disease; and I venture to hope that these discussions may assume that particular form which will prove valuable to individual physicians who are battling with disease in the smaller towns, hamlets, and remote points in the land, and who are known and properly designated as family doctors. These are the men who need your aid, who deserve the stimulus of your best thought and the inspiration of your example. As a final word, let me enter a plea in their behalf to the end that your literature be sent to them with the liberality so characteristic of your organization, and that they be invited, when practicable, to attend your meetings.

Mr. President, it remains for me to thank you for the distinguished honor you have conferred upon me in inviting me to address this Academy at this time. I appreciate it more than mere words can express. Yet I regret that I have so inadequately fulfilled the responsibilities of the high office you have so kindly thrust upon me. But I assure you, sir, that I shall carry with me down to the crack of doom the memory of this pleasant visit to the New York Academy of Medicine.

DISCUSSION.

THE PRESIDENT.—The chairman congratulates the Academy on the felicitous manner in which this suggestion by Dr. Potter, that we devote ourselves more extensively to the consideration of the causes of disease, has been presented, and that the paper has brought out so many members who are capable of discussing this important subject in the most proper manner. I think it is possible that in the activity of our practical work which, as has been suggested in the paper, could well be cared for by the several sections, we may at times forget the great current—the great stream—in which the practice of medicine ought to flow, the one which shall lead to the broad ocean of peace: to the protection of the community from preventable disease.

DR. A. JACOBI.—I first wish to assure Dr. Potter that the applause which was given when he finished his paper was not intended to emphasize his eulogy of the Academy of Medicine, but was meant for him.

There are a number of points in Dr. Potter's paper that are worthy of discussion at some length, but I shall only allude to one. He spoke of suppressing contagious diseases by means of law or laws. We all know how difficult that is. We all know how strong the objection is in our community against the enforcement of any law which tends to infringe on what we call our individual rights. Thus it is that whenever a law pointing to the enforcement of certain sanitary rules is proposed, there is at once a general outcry. It will prove a very difficult matter to pass such laws. We shall have to come to it, certainly, for although I am addicted to the same tendency of asserting my individual rights as a member of a republican community, still I know, and all of us ought to know, that the general welfare is of greater importance than the apparent rights of an individual. We must all submit to general rules, but it is a very clear fact that in the enforcement of health laws there is more pronounced opposition than in the enforcement of laws of any other character. All of us ought to do what we can, through the board of health, through the legislature, etc., to further the enactment of health laws for the general welfare. Through our personal efforts, we can do much more, perhaps. Every one of us commands the attention, the respect, the obedience, of say ten, or twenty, or fifty, or one hundred families. By our individual efforts among these families, we are benefiting the public at large. There are many instances in which disease can be prevented by simply directing the necessary attention to the individual hygiene and health. Many diseases will not take hold of a healthy individual. As long as an individual is in good health, tuberculosis or diphtheria has no power over him; he cannot even become infected with cholera; he may swallow ever so many cholera bacilli, or eat raw fruit that is covered with them; he may inhale or swallow ever so many tubercle bacilli, or nurse a case of diphtheria—so long as the mucous membranes are in a healthy condition, he will not become infected with any of these diseases. So long as the mucous membrane of the nose and throat is covered with normal

epithelium, there is no probability of a diphtheria germ getting in there. The only exception to this rule is in the tonsil cases where there are, according to Stacke, small interstices between the pavement epithelial cells. The rule is, that there is no part of the entire mucous surface of the nose, throat, and the remainder of the respiratory organs that is not fully covered by epithelium. Only those persons become infected with diphtheria who have a catarrh, either acute or chronic, who have large tonsils or who have had tonsilitis a number of times, or those in whom the epithelium is broken from other causes. We should impress on the minds of the heads of families that no so-called slight cold, particularly during a prevailing epidemic of diphtheria, must be neglected. So long as the baby has a running nose, there is a possibility of the diphtheria germ being deposited there. For that reason, a running nose should be cured as quickly as possible. So long as the child has hypertrophied tonsils, or tonsils in a condition of chronic inflammation, there is a possibility that the diphtheria germs will be deposited there and thrive. See to it that every child in your clientele has healthy tonsils. Where they are swollen, either congenital or acquired, resect them. It is a trifling operation usually, and in cases where there is no diseased tonsil or only a healthy cicatrix, there will be no diphtheria. Why is it that we see so little diphtheria of the tonsils in adults? It is because their epithelium is stronger, and there is a comparative absence of hypertrophy of the organ, or there is a good deal of dense cicatricial tissue in them. In removing the tonsils, it is better to cut away a large portion and get a good, healthy cicatrix. If, in the families under your care, you have a large number of cases of diphtheria, it is your own fault and the fault of the heads of the families. In many instances where the parents refused to have the nose and throat of the children attended to, I have seen diphtheria invade such families and decimate their number. I have seen many cases of diphtheria in this city during the past thirty-six years, and I repeat that as long as the mucous membrane of the respiratory tract is in a healthy condition, and as long as there are no deep follicles in the hypertrophied tonsils, there will be no diphtheria.

The same is true of tuberculosis. We live in an atmosphere that is probably filled with tubercle bacilli. I trust that very few of you are tubercular, but every one in this room has swallowed and digested tubercle bacilli by the hundreds and thousands. So long as the mucous membrane of the larynx and lungs and intestines is in a healthy condition, you will not develop tuberculosis. The ciliated epithelium will quickly get rid of the bacilli. They cannot get in between the epithelial cells, or enter the circulation. It is not uncommon to see cases of tuberculosis which first develop in the larynx. Such can only be explained by the fact that when those persons were exposed to the bacilli, they had a laryngitis, such as I have now, and the germs found a favorable resting-place and produced ulcerations. Then it may take another six months or longer before the lungs or glands become involved.

These remarks are also true of cholera. You may live in a cholera-infested country and swallow ever so many bacilli; they will do you no harm so long as the mucous membrane of the stomach is in good condition, because the acidity of the gastric secretion will kill them. Pettenkoffer and others have proven this. When they swallowed the bacilli of cholera, they simply gave rise to a little irritation of the intestines; nothing more.

Every one can do a great deal to prevent the spread of the very worst forms of contagious diseases by seeing to it that those who are under his care have healthy integuments.

DR. J. WEST ROOSEVELT.—If I may be permitted, I will take up this subject where Dr. Jacobi left off, and speak of what the medical profession can do in the way of actual, practical work toward the furtherance of legislative measures. As Dr. Jacobi said, physicians work as individuals. Each person here has a larger or smaller number of people to whom his advice is a matter of more importance than he realizes. There exists no more powerful body for moulding all sorts of public opinion than the medical profession. What the medical profession as a body want, they can have, so far as our legislature is concerned. By rational, concerted action they can do much to shape the policy of the state and preserve the health of the public. One of the most serious problems that those

of us who wish to improve the general sanitary condition have to face is, that the first ones who should be instructed are the physicians themselves. I do not mean to imply that the doctors do not know the laws of health; but there is a vast difference between knowing something and realizing it. How many of us will think it worth while to preach, talk, and impress on people the facts that have been brought out this evening? How many of us actually realize and talk in a tangible way of the actual danger of becoming infected with tuberculosis from a given patient? Perhaps the danger of thus becoming infected has been exaggerated and in this way given rise to a great deal of doubt, both in our own ranks and among the laity. The tuberculous person is not all that we have to fear. Tuberculous food is not all that we have to fear, but there are certain tubercular people and animals which become very dangerous to the public health. When we come to isolation, and talk of stamping out tuberculosis in that manner, we are proposing something so monstrous, when we consider its details, that it is perfectly hopeless to try to influence any legislature to pass such a law, and worse than that, it causes a very distinct feeling of reaction against the enactment of health laws in general. To deliberately propose to isolate over one twentieth of the population of New York city, because they are tubercular or thought to be so, would kill the entire scheme. It is a mistake to first pass a law and then teach the people what it means. We must first decide what we want, and then talk about it to every one we meet. These ideas should first be propagated among the people. Get people to understand that certain sanitary laws are beneficial to them, and then they will demand their enactment.

A few more words regarding the dangers of infection from tuberculosis. It is well known that for every one who has consumption, millions have been exposed without taking the disease. To fight the bacilli seems almost a hopeless task. If billions of these germs were destroyed daily, the number left would still be sufficient to keep the disease very much alive. Of course, that is no reason for not fighting them, but it shows the magnitude of the work before us. It reminds me of a story I once told at a medical society. I was once talking with a gentleman in

India, where snakes are very abundant. This man had a perfect furor for killing snakes. He had lived in India for twenty-five years, and during that time he had killed at the rate of twenty-five or thirty snakes daily. Still, they were as abundant as ever. When I spoke to him about the hopelessness of his task, he quite agreed with me, and added, "Still, I object to having snakes in my parlor."

The germs of tuberculosis are so constantly present, and so numerous, that we must fortify the body against their development.

DR. S. S. BURT.—I have listened with much interest to the paper and discussion. With the sentiment of Dr. Potter's paper I am very much in accord. I scarcely ever write a medical paper in which I do not touch upon the prevention of the disease. That seems to me a very important part of the practice of medicine. In all my teaching I try to inculcate the same idea. It is our individual work that is going to do the most good. We cannot legislate for people who do not want to be legislated for. We must get people to demand that things should be thus and so; then it is easy to make the laws. We must spread the knowledge we now possess about disease, its limitations and preventability, and I think the work of the bacteriologists is of the utmost importance in that direction. Many of them are seeking for remedies for diseases; but in their work they find out how diseases are disseminated, which seems to me to be the great result of their labors. It was so with Koch; it was so with other men who have discovered the specific germs for certain diseases. While we cannot kill all the germs that cause these diseases, we can do much to prevent persons who are susceptible from coming in contact with them. I believe, as Dr. Jacobi said, in building up the patient's general condition. After the disease is taken, the best thing we can do is to build up the system, so that the patient is able to resist and even crowd out the germ. In the treatment of tuberculosis, you will find one man giving creosote, another man, inhalations or injections, and so on; but it seems to me the best thing we can do is to fortify the system against further progress of the bacilli, and make it a poor nidus for their development. It may be that in the future certain remedies will be discovered to counteract the effect of the

germs; but at the present time, none such exist. That we can fortify the system so that the germs are harmless, is undoubted. That is the line I have always taken and tried to instil into my students. In our association with our patients, we should tell them what we know on this subject, and in that way we are doing much to improve the condition of the community. It will take a long time to make the public understand it, but we should not grow impatient, for progress is always slow. We may not live to see it, but the time will surely come when the community will be sufficiently educated to demand legislation on these matters. You cannot legislate against a man's drinking, if he wants to drink. When people demand the laws, then they will also obey them.

Tuberculosis can best be prevented by building up the general condition. I have been exposed to the disease for seventeen years—probably as much as any other man. During these years, I have been in attendance at the worst dispensary in this city,—the Bellevue Out-door Department,—where we are constantly exposed to tubercular infection, but I did not get the disease. I think this is due to the fact that I took great pains to keep myself in good condition. We can do this by means of diet, exercise, etc. We can do more than that: we can prevent, in many cases, exposure to the disease. In typhoid fever, where the bacilli are carried in the excreta, we can prevent them from getting into the drinking water and the food. I think small-pox has been crowded out quite as much by isolation and disinfection as by vaccination. I believe that other contagious diseases will gradually be wiped out in the same way rather than by inoculation or vaccination.

DR. JOSEPH D. BRYANT.—I desire to say to Dr. Potter that I have no doubt that, as the result of his paper and of the discussion it has provoked, a new stimulus will be given to this line of thought. It may not be unknown to all present that it was my duty for some six years to be health commissioner of New York city. I remember it well. About three or four years ago, while occupying that position, it occurred to me that it would be very wise to open a war on tuberculosis, and after consultation with Drs. Prudden, Biggs, and H. P. Loomis, a circular was formulated and addressed to the profession, through the

medical press, and incidentally it gained access to the secular press. I also sent a circular letter on the subject to twenty-four leading physicians in this city, asking whether, in their opinion, with our present knowledge of tuberculosis, the board of health would be warranted in assuming it to be a communicable disease, and in requiring that it be reported like other infectious diseases, and in taking steps to combat its advance. That was three or four years ago—I think four. Dr. Jacobi will remember it, as he received one of the letters. Of the twenty-four physicians to whom this circular was sent, only six replied; Dr. Jacobi was one of them. I received two replies promptly, with every question answered in the affirmative. Without going into any further details, suffice it to say that the health commissioners came to the conclusion that there was not sufficient support from the medical profession to warrant its adopting such radical measures in connection with tuberculosis. It was then decided to "back-track" the disease, which means that, whenever a death was reported from tuberculosis or from any disease associated with it, the case was referred to the contagious disease department, and inquiries were made regarding the surroundings, etc., of the patient, and whether a previous case of tuberculosis had occurred there.

Now comes in the educational part. Only a few years have passed since then; but now we find that every physician of prominence—and every other physician, so far as I know—is willing to assume that great good can come from efforts directed towards the isolation of tuberculous patients, and we even hear the proposition made that a law should be enacted for the control of such cases.

There is a law now in force in this state requiring the destruction of tuberculous cattle. I think I may be excused when I say that I am responsible for the framing of that bill. It was drawn up at my request by the attorney of the department; it was then taken by me to the state board of health, of which body I was a member. I assured them that the tenor of the bill should become a part of their duties, and requested them to endorse it. In spite of vigorous opposition, the bill was finally passed. While I am not at all familiar now with the details of the methods that are employed by the inspectors, still the pro-

visions of the bill regarding diagnosis, etc., are very broad. It is true that the owner loses the cattle, but it is equally true that tuberculous cattle are of no value to any one.

DR. W. W. POTTER (closing the discussion).—There has been so little said in criticism of what I have offered, that it is hardly essential for me to take up any more of your time, after having been allotted a considerable part of the evening. I agree with the gentlemen who have spoken, and I think they also agree with me; yet let me accentuate one or two points brought out in the discussion. I did not come here for the purpose of asking this Academy to begin by petitioning the legislature or any municipal body to enact statutes or ordinances relating to the prevention of disease, but I did come here to ask you to enter into a campaign of education; this the New York Academy of Medicine is competent to do and constantly is doing on all important questions relating to medicine. Hence I venture to ask you to enter into such a campaign regarding the prevention of disease.

The pendulum has swung very far towards specialism in medicine, and your various sections, so well equipped, emphasize that fact, all of which must meet the approval of all intelligent men; but let us come together occasionally in our general meetings, here and elsewhere, whether we are specialists or not, and discuss questions relating to the prevention of disease. To prevent disease is certainly a very high aim for a physician. Let us, too, remember that physicians in the villages and small country places throughout our vast commonwealth need just such support and just such advice as you can give them by your discussions, in order that they may intelligently educate the communities in which they live with reference to the prevention of disease. The enactment of statutes for that purpose is of secondary importance. These may come, if needed, after the people appreciate their necessity or importance. As Dr. Roosevelt has so justly said, let us begin by talking about these matters to our neighbors. Let us make them understand that the highest function of a physician is not to prescribe a dose of medicine, but to endeavor to prevent disease. Dr. Jacobi has taught me a valuable lesson with reference to the prevention of diph-

theria. I had no conception that it was within the domain of the physician to control that disease so effectively as he intimates.

Dr. Bryant deserves the thanks of the community for his action with reference to the inspection and condemnation of tuberculous cattle. There are so many sides to the question, and so much to be said on all of them, that I hesitate to dilate upon them, lest you become wearied.

Stated Meeting, February 15, 1894.[1]

SCORBUTUS IN INFANTS.

By WILLIAM P. NORTHRUP, M. D.

The pediatric section comes before the general session of the Academy on the invitation of our honored president. Our topic is "Scorbutus in Infants." What is our excuse for presenting this topic in a meeting of the general profession? The answer is, pediatrists have recently learned to recognize a disease and believe they have saved a few lives by recognizing it. To impart this experience to the general practitioner is the object of this discussion. It is their belief that the wider distribution of this knowledge will help to shorten suffering and save yet other lives. The first case the writer ever saw died. Its autopsy did something for the recognition and classification of scurvy in the United States. A discusser of the topic to-night will tell you of two patients in a Southern town. One developed curvature of the spine and for that was brought North. On antiscorbutic treatment it promptly recovered. Its fellow at home without treatment died. A frank confession will bring to light many cases that have continued for many months on general tonics, and at last dragged themselves to slow recovery. To advise from our experience, to add something to the general stock, to help in a definite line, is our specific errand.

Some may say, sailors used to have scurvy before they took lime-juice to sea. Soldiers had scurvy in outlying posts where they lived on canned food and lacked fresh vegetables, but can it be possible that children and infants

[1] In charge of the Pediatric Section.

also suffer from it in New York? You will hear the speakers say that it has been observed not only in New York, but in Boston, Philadelphia, Cincinnati, and elsewhere. It appears in the great cities and in the country, in the homes of wealth, and in the crowded tenements of the poor.

In 1873 a case of infantile scurvy was reported in Virchow's *Jahresbericht*. In the *Lancet* of November, 1878, Dr. W. B. Cheadle, of London, reported three cases of scurvy supervening on rachitis in young children, and again in the *Lancet*, in 1882, he published a paper on "Osteal or Periosteal Cachexia and Scurvy." At the Copenhagen International Medical Congress, Professor Rehn, of Frankfort, reported seven cases of scurvy in children. Thereafter, a few cases of cachexia thought to be scorbutic were observed in the United States by Jacobi and Forchheimer. These cases were, perhaps, not typical and never found their way into indexed literature. When shown at the Foundling Hospital, in 1889, a child suffering with scurvy, the writer groped among several diseases for an explanation. No diagnosis having been made the baby was not treated for scurvy, and not being treated for scurvy it died. The post-mortem findings revealed the true nature of the disease.

Two years later the writer met with a typical case in consultation. This was in rich surroundings; was instantly recognized, treated, and quickly cured. These two cases compass the entire field. One, a foundling, undiagnosticated, came to autopsy showing typical lesions. The second, early diagnosticated and promptly treated, was better in five days, well in a month.

The first might be an institution curiosity, but the second brought the disease into the field of the general practitioner. Being suspicious that scurvy might be of occasional occurrence in general practice, the writer, in 1891, set about finding cases like the two in his possession.

The surgeon-general's office could furnish but one possible, though not typical, case. In New York and vicinity, seven well-marked cases by dint of search were added to these two with two more less definite, making a total of eleven. These eleven cases were reported in a paper read before the American Pediatric Society in September, 1891.

Scurvy is plainly a disease of malnutrition directly or indirectly dependent upon prolonged use of improper food, or the absence of fresh food or fresh-fruit juice. Its most characteristic symptoms are spongy gums, and tendency to hæmorrhages. Notably there are subperiosteal collections of blood about the femora and other long bones, escape of blood into and beneath the skin, forming petechiæ and ecchymoses, into the mucous membranes of the mouth, intestines, pelvis of the kidneys, giving rise to external hæmorrhages, hæmatemesis, bloody passages, and hæmaturia.

CASE I.—Female; foundling; sixteen months old, part breast-fed and more slop-fed; "because of impaired nutrition," as history says, when seventeen months old, and one month before death, was transferred to hospital for treatment. Right leg and knee swollen, and extremely tender; temperature, 101° F.; two weeks before death child appeared very sick; gums swollen, smoky black, bleeding easily; two days later, eyelids swollen, ecchymotic classical "black eye" in appearance. In the last week of life it developed pneumonia, a second "black eye," swelling of companion thigh, temperature still 101° F. During the last days the patient became extremely anæmic, having a metallic pallor against which stood out these two "black eyes," giving a particularly horrible appearance. If anything more was needed to transform a fair child to a grinning gargoyle, it was then found in the dried fringe of black blood about the lips. There were no petechiæ; passages black and pasty.

This infantile cachexia had these symptoms—marked anæmia and hæmorrhages in parenchyma of lungs; hæmorrhages into the eyelids; hæmorrhages into the bow-

els; hæmorrhage into the mouth. In short, it was hæmorrhage, more properly diapedesis and anæmia.

We have described a poor waif, without previous family history, cared for by an hireling, improperly fed, which died of cachexia, multiple hæmorrhages due to malnutrition.

Here is another, taken sick at the same age, sixteen months. Its surroundings embodied every refinement of intelligence and wealth. Its father was a ruddy athlete, mother of apparent good health; both in the thirties. After the fourth month the mother's milk failed, and the child was fed on something made to sell, labeled prepared baby's food.

For a time it thrived, apparently, though backward in walking and talking. Its digestion was good, bowels regular, and it seemed satisfied with its food. No evidence of rickets; teeth in usual quantity appeared at the usual time.

After twelve months of this feeding on a patent pap, mixed up with milk and water, the nurse noticed a change in the gums; one week later, trouble in the right, lower limb, worrying, pain on handling, and a tendency to keep the limb nearly straight. The diagnosis was rheumatism and it was treated accordingly.

The child cried on seeing a strange face, becoming alarmed also for the safety of its lame leg. In the wry face of crying, the little patient fairly unbuttoned from between its lips two rows of irregularly nodulated purplish gums, from the summits of which the points of its teeth barely protruded. In the upper spongy row was a depression with ulcerated walls and sloughing shreds. The gums were dark and bled freely in the act of crying, from compression of the lips alone. There was nothing further abnormal about the face beyond a worried expression.

There was no history of nose-bleeding, no hæmaturia, no hæmorrhages from the bowels. The child was now stripped of all clothing and laid on its back on the bed. It continued to whimper, throw its arms about freely, draw up its left leg; as for its right, it could move it slowly, but only a little, and could not be induced to flex it. The right thigh was somewhat larger than the left. The enlarge-

ment was fusiform, greatest just above the knee. Apart from the spongy gums and swollen thigh, there were no external manifestations. The diagnosis was to be made on the lesion of gums and thighs.

Let us see the corroboration of the diagnosis of scurvy. The child was removed at once to the country, its proprietary mixture was stopped and in its place were given fresh cow's milk, expressed juice of beef, baked potatoes. The one thing which this scurvy case seemed to crave, for which it reached out, which it seized with ravenous avidity, was orange. The child could hardly be restrained till it held the fruit in its grasp; and then proceeded to souse its lips and nose in the juice. Improvement began at once; in five days its gums were markedly better, in ten entirely normal. As for its thigh, at the end of ten days the improvement was marked both in sensitiveness and size, and a month later the child was standing on its feet. A slight thickening over the femur could be detected for a few days, at the end of which time the patient seemed absolutely well.

In the paper before mentioned (Washington, September, 1891), were narrated eleven cases of scurvy in children. The observers were Drs. R. Van Santvoord, A. Caillé, C. H. Richardson, Herman Goldenberg, F. Delafield, L. E. Holt, of New York; Drs. W. F. Lockwood, of Baltimore, and G. H. Whitcomb, of Greenwich, Conn. As many of these gentlemen are expected to be present to-night, I will refer but briefly to their cases.

Eleven cases collected, after correspondence with twenty-five members, etc. Of twelve cases, the ages ranged, in eight of them, from 11 months to 18 months, the others being $2\frac{1}{2}$, $2\frac{8}{12}$, 6 years. Temperatures not high; 101° F. so far as noted.

Causes attributed—(1) "Proprietary food," with water and milk; (2) wet-nursed and fed by paid nurse, nursing her own and another (a scurvy and fatal case); (3) condensed milk; (4) proprietary food; (5) exclusive proprietary food for seven months; (6) exclusively on artificial food; on proprietary food exclusively for two months;

food of a boy two years old, dispensary patient, exclusively of milk; exclusive diet of meat for three months, by doctor's order for cure of diarrhœa, remedy continued indefinitely; lastly, unknown, three, of which two were fatal cases.

It would seem that a prolonged unvarying diet, exclusively of meat (child three years), or milk (child two years), can produce this malnutrition. Among the flickering bacteriological possibilities may be mentioned a report of Balies. In two patients he succeeded in isolating a slightly curved bacillus pointed at the extremities, etc. Two rabbits inoculated with an emulsion of the affected tissue, under proper precautions, produced hæmorrhagic extravasations in various parts of the body. The organisms could not be found in the fluids of the body. Streptococci were usually associated with these bacilli. Inoculations with a pure culture of the bacilli in large quantities gave rise to abscesses, subcutaneous and subserous hæmorrhages, and death. The opinion is expressed that the organism is one usually present in the mouth, and that in depression and privation it finds the conditions favorable for its growth. Its detrimental action is thought to be due to the generation of an agent that exerts a poisonous influence upon the blood vessels and blood corpuscles. All of which requires further proof.

Lesions.—Spongy gums, swollen thighs; swelling both thighs, gums smoky and bleeding, "black eyes," bloody passages, anæmia, pneumonia; spongy gums, swollen thighs, demonstrated subperiosteal hæmorrhages; petechiæ; spongy gums, one ankle swollen, tender, bluish; swollen, purplish, bleeding gums, bloody stools, ecchymoses of legs; spongy gums, legs flexed and rigid (subperiosteal hæmorrhages); spongy gums, purple and bleeding, swollen knee and thigh; spongy gums, ecchymoses, bleeding from mouth, bowels, and bladder; spongy, bleeding gums, pain on handling legs.

Extensive subperiosteal hæmorrhages with separation of epiphyses of both humeri in an idiot six years old, fatal, and two fragmentary reports; hæmorrhages, but no note of gums.

Cases fatal of the twelve were four.—The first, second, and third, no note of the treatment; fourth, vegetable acids.

Successful treatment:

Orange juice—gums normal in ten days; legs normal in thirty days.

Orange juice, lemon, fresh milk, "live diet"; improved in three days; lost track of.

Muriatic acid, tincture of iron; "gradually recovered."

Orange, peach, fresh milk undiluted; in a few days improved, no second visit.

Orange, rare beefsteak, fresh milk; three weeks, entirely well. This case was treated for "severe rheumatism" for twenty-four days without improvement.

Lemon juice, fresh vegetables (child two years), chlorate of potash for gargle; well in "few days."

"Rational diet," brief notes.

Orange juice, immediate improvement. Had been treated with $KClo_3$ internally (gr. v. t. i. d.) for four days; no improvement; "craved," "cried for orange."

A word concerning the hæmorrhagic diseases according to the authors—in this instance Strümpel. In purpura simplex, the hæmorrhages are mainly in the skin, the mucous membrane and deeper tissues remaining intact, gums unchanged.

In purpura rheumatica or peliosis rheumatica of Schönlein when hæmorrhages are attended with dragging rheumatic pains, and constitutional disturbance, there may be actual arthritis, and mostly of the knees and other joints of lower extremities. The gums are normal and no hæmorrhages in mucous membranes.

Morbus Maculosus (Werlhofii).—Here we have a grave

condition. The cutaneous ecchymoses are extensive; there are hæmorrhages from the mucous membranes of the nose, mouth, soft palate, stomach, and intestinal canal, serous membranes, possibly into the brain and kidneys. The general condition may be "typhoidal," but the gums remain intact.

As Strümpel says,—"The various 'hæmorrhagic diseases' are so intimately related to one another that it is quite impossible to make a categorical division of them. There are conditions in which hæmorrhage seems an important part and all merge into one another."

To summarize what may be gathered along the lines of experience and upon the points of the programme, we may again define our topic. Scurvy is a condition of malnutrition, which affects the capillaries or corpuscles, or both, allowing blood to escape from the vessels. This is a condition whose anatomical changes I am not prepared to give. The collections of blood occur most often about the shaft of the femora and beneath the periosteum, next in petechiæ and ecchymoses of the skin, often in the eyelids. A similar process may take place upon any mucous membrane, giving rise to epistaxis, bloody stools, hæmaturia, and hæmatemesis. There is consequent anæmia. Points of tenderness, notably about the femora, near the knee, mark the elective location of greatest subperiosteal hæmorrhage.

Etiology.—Most cases have been attributed to persistent feeding of infants upon patent prepared foods and condensed milk. Even a small proportion of cow's milk, not condensed, and even breast milk in small proportion have, according to some observers, not sufficed to protect against the malnutrition. It occurs among the rich where a rigid regimen has persistently excluded fruit juices, and among the very poor.

A collection of cases thus far shows the age most subject to the typical disease is the second year of life.

The main, typical symptoms are swollen and painful thighs and spongy gums.

The treatment—orange-juice and other fresh fruits.

The disease is believed to be a distinct entity, though often associated with, or superadded to, rickets. Formerly it was believed to be so intimately associated with it as to deserve the name of acute rickets.

SCORBUTIC PSEUDO-PARALYSIS.

BY HENRY LING TAYLOR, M. D.

The title, "Scorbutic Pseudo-Paralysis," was chosen for these remarks in order to emphasize a peculiar and prominent symptom, frequently occurring in the cases of infantile scurvy, and well marked in a case of my own, an outline of whose history follows:

A little girl eleven and one-half months old was brought to me from a distance for treatment last August, as it was believed that she was suffering from some spinal or joint affection. This baby had been fed exclusively on a special brand of condensed milk, upon which she had seemed to thrive up to the age of six or seven months; at six months of age, she was rosy, and weighed twenty-five pounds. The local symptoms, noticed in their order of appearance, were: weakness in sitting and a peculiar, bent attitude; later, inability to sit up; indisposition or inability to move the legs, and later, the trunk; a tender swelling on the right thigh, disappearing in a few weeks; spongy and bleeding gums; a "heat rash" over the body; swelling about one eye, disappearing in a week or two; a few purplish spots; and a swelling above the right ankle. During the development of the symptoms, the baby's nutrition became much impaired, and she lost appetite and flesh, and slept badly; she also became peevish and excessively sensitive to the slightest touch or movement. For several weeks before her visit, she had been very weak and cross, and had perspired profusely. At no time had there been noticeable disturbance of the stomach or bowels. At the time of examination, her condition had become critical, as she was very weak and had almost ceased to nurse. The rectal temperature was 102½°. The skin was dusky and covered with a papular eruption; the face had an anxious expression, indicating severe and continuous suffering. The baby resented with loud outcries any attempt to handle or even approach her, and in crying she exposed

her purplish, swollen gums, from which the eight incisors protruded; in the vicinity of the teeth, and particularly on the upper jaw, the gums were spongy and ulcerated, and it was noticed that the breath was fetid. She lay in a bent attitude with the legs somewhat flexed, and made no attempt to move them. There was, however, motion at the joints, and when the soles were tickled, the toes of both sides were moved and the ankle on the left side; the knee reflexes were normal; there was a tender swelling of moderate size above the right ankle, not involving the joint, and motion and pressure on the lower limbs were especially painful, though the whole body seemed sensitive to contact. The arms and head were moved naturally by the baby. The spine was abnormally rigid and somewhat bent, so that there was a distinct projection at the first and second lumbar vertebræ. There were no symptoms of rickets. The whole clinical picture was as distinct as it was striking, but the fact that scorbutus was suspected, was due to my having read Dr. Northrup's excellent paper on the subject. The diagnosis was made positive the same day by my friend, Dr. J. Henry Fruitnight, whom I called in counsel. The mother was directed to place the baby on a cot and on no account to move her, and the baby was at once put on a diet of pasteurized milk and fed every three hours; she also received the juice of one orange and two teaspoonfuls of raw beef juice daily. From barely tasting the milk, she took more and more at each feeding, until in a day or two she took six ounces, and this soon had to be increased, as it failed to satisfy her; the beef juice and orange, and especially the orange, she took greedily from the start. No drugs were exhibited, except an occasional dose of phosphate of soda as a laxative. The little sufferer began to gain at once; she became quieter and evidently experienced great relief, for she slept soundly through the first night, which she had not done for six months. The fever abated one and one-half degrees a day until it reached normal; and in a very few days she lost her peevishness, the eruption began to fade, and the gums to heal, and she commenced to use her legs. Within three weeks, the spine became straight and flexible, the swelling above the ankle had subsided, the baby was able to sit up and to move the legs

freely, and her general condition being satisfactory and all the symptoms having disappeared, she returned home a few days later, and was well when last heard from, a short time ago.

The parents of my patient report that a neighbor's child that had been brought up on the same brand of condensed milk, was apparently healthy and well-nourished till the age of eleven months, when painful swellings made their appearance near the joints of the lower limbs, the legs lay motionless and helpless, and the child was in great distress, especially when moved. The gums were dark and swollen and were said to have bled. This infant died exhausted in about two months.

Of the many practical points suggested by these observations, but two or three call for special mention at this time. The cause of the disorder is clearly to be found in the diet, and condensed milk must be added to the long list of infant foods that may prove dangerous. These babies appeared to thrive famously for many months on their condensed milk, and the state of malnutrition came on without special disturbances of the digestive organs, so that the attendants, both medical and lay, were entirely off their guard in the matter of diet. The parents were well-to-do people, and infants of this class seem more liable to this disorder than those of the poor. This may be due to the fact that the latter are more likely to get fresh vegetables from the family table, as suggested by Cheadle, and are less likely to get the artificial foods. It should not be overlooked that when a baby that has been sitting up normally ceases to do so, or when a child fails to sit up at the proper age, the cause should be searched out. The inability to sit properly was the first symptom to attract attention in my case, though she had previously sat up straight, and without support. The peevishness, restlessness, and general and local suffering are important symptoms, due, no doubt, in part to the blood and tissue changes, and in part to local bloody and serous infiltration or effusion, and particularly to hæmorrhages beneath the

periosteum of the bones of the lower extremities, the ribs, thorax, pelvis, or skull. The pseudo-paralysis is one of the most striking of the group of characteristic lower limb symptoms in these cases; the trunk and upper extremities may also be affected.

Inability to sit up, and varying degrees of helplessness, are frequently mentioned in the histories of scorbutus in infants. Taking only cases of two years and under, we find, in Barlow's and Northrup's papers, such expressions as the following: "Immobility of right lower limb;" "scarcely moves any part of body except head;" "right arm drops as if paralyzed;" "legs hung down in a helpless fashion;" "lower limbs moved voluntarily very little;" "drew up its left leg; as for the right, it moved it slowly, but only a little, and could not be induced to flex it;" "right leg semi-flexed and everted;" "the legs were partially flexed, and remained rigid." The limbs and trunk usually lie motionless, as if powerless, and the baby makes no attempt to change its position. The head, and usually the arms, can be moved, and even in the affected members there are commonly certain movements that can be executed with more or less difficulty, but even handling which caused acute suffering, and tickling of the soles failed, in my patient, to evoke the smallest movement of the knees, hips, or trunk.

As certain members can be moved, the pseudo-paresis can not be wholly due to the prostration of the patient, though this and the disinclination to effort of any kind, characteristic of the scorbutic state, doubtless intensify the condition; neither is it to be entirely explained by the local hæmorrhagic lesions, for while there was evidence of but one such lesion in my case, namely, that near the right ankle, the entire left leg, except the foot, was perfectly motionless. Just as little can we assume any essential paralysis, for the knee-jerks were normal, and recovery took place in a few days. I ask, therefore, if bloody and

serous infiltration into and between the muscles, and also the probable loose and succulent condition of the deeper layers of the periosteum, in certain locations, due perhaps to the more rapid growth of certain bones from the sixth to the twentieth month, and determining the site of the characteristic swellings, may not give rise to the bent attitude and pseudo-paralysis, since contraction of the belly of the injured muscles, and traction on their tender periosteal insertions would be instinctively avoided. In other words, is movement inhibited by the sensitive condition of the muscles and their insertions, or must malnutrition of the nerve centres from impoverishment or toxicity of the blood be assumed? Where joint irritation exists there would be local reflex muscular rigidity, which would also operate to prevent motion, as would also peri-articular and intra-articular swellings.

This pseudo-paralysis is easily distinguished from other infantile paralytic affections by the exquisite tenderness and other symptoms of the scorbutic dyscrasia, by the normal knee-jerks, and by its speedy subsidence on anti-scorbutic diet. A somewhat analogous condition is the pseudo-paralysis of rickets, so well described by Dr. H. Berg. As scorbutus may coëxist with rickets, and may also be mistaken for it, it would be well to have the symptoms of scorbutus in mind, when cases of supposed rachitic pseudo-paralysis come up for consideration.

The scorbutic swellings due to sub-periosteal hæmorrhage are frequently mistaken for acute rheumatism, and might be confounded with periostitis, acute arthritis of the hip, knee, and ankle, and acute abscess, but differ from all these affections in the absence of local elevation of temperature; fever may be present or absent. Certain cases, like my patient's, might be mistaken for spondylitis. It also seems not improbable to the writer that some of the obscure cases of infantile malnutrition with weakness, inability to sit up, bowing of the spine, and perhaps low

fever, oftener due to chronic intestinal inflammation or to rickets, may be masked or incipient forms of scorbutus.

The peculiar combination of the lower limb symptoms, including the pseudo-rheumatic and pseudo-paralytic manifestations, the turgid and spongy gums, and especially the rapid improvement on anti-scorbutic diet, is characteristic of this affection. It should be borne in mind, however, that one or more of the symptoms, and particularly the gum symptoms, may be absent in early or aberrant cases; but peevishness, exquisite tenderness, inability to sit up properly, and cachexia are usually noticed, and the whole forms a symptom-picture so striking that a correct differentiation from affections having points of similarity should not usually prove difficult.

REMARKS ON SCORBUTUS IN INFANCY.

By LOUIS STARR, M. D.,
PHILADELPHIA.

Dr. W. P. Northrup's most valuable paper on "Scorbutus in Infancy," read before the American Pediatric Society on September 24, 1891, and published in the "Archives of Pediatrics" for January, 1892, was the first attempt by an American author to establish a definite position for, and explain the origin and treatment of, a well-defined complex of symptoms, a group of conditions which, in my experience, confused many physicians, being classed sometimes as purpura, sometimes as acute rickets, or rheumatism, or allowed to pass entirely unplaced, and often, in consequence, uncertainly and unsuccessfully treated.

From time to time in my private and hospital work I had met with infants presenting a mild type of the condition which I would now term scurvy, but it was not until the year preceding the appearance of the above mentioned paper that I had the opportunity of studying four well-marked examples. The first two cases I was at a loss to accurately classify, but they had been improperly fed, and quickly recovered when placed upon an improved diet. The third presented so many points in history and clinical features suggesting scorbutus as met with in adults, that both Prof. J. M. DaCosta, who saw this child with me, and myself inclined to this diagnosis. This patient, seen late in the course, subsequently died. The findings at the autopsy corresponded exactly with those observed by Dr. Northrup in his fatal case, reported in the paper referred to. The fourth case I saw in consultation, and diagnosed as scorbutus. I was, therefore, much gratified on reading Dr. Northrup's article later to find that he had established

for this condition a distinct place among the diseases of childhood.

Since 1891 I have seen nine cases, making a series of thirteen, all of which recovered with the exception of one, the case already alluded to. All of them presented the clinical features so graphically described by Dr. Northrup and Dr. Thomas Barlow of London. In fact, I know of few diseases in which the various symptoms are so constant in their grouping and presence. I have found the following conditions:

1. Immobility, excessive tenderness, and a varying degree of swelling of the lower limbs. The swelling was most marked in the neighborhood of the joints, and upon subsidence revealed a thickening of the shaft of the bone. The skin over the swelling was shining, tense, and often livid or purplish in color, but did not pit upon pressure, and was normal in temperature. There was liability to fracture near the epiphyses; this was marked in my fatal case.

2. A purplish discoloration of the gums, which were also often spongy, very swollen, and bled readily. These features were *entirely absent* when no teeth had been cut, and when a few teeth only had appeared they were *absolutely limited* to the gums bordering these.

3. Complete and rapid recovery following the adoption of an anti-scorbutic regimen,—a rule with but one exception in my experience.

Each of my cases occurred in the best of surroundings and among people who could well afford all the luxuries of life, and each was definitely traced to improper feeding, the fault lying usually in the employment of some "baby food" as a substitute for breast milk, or in place of a properly proportioned cow's-milk food. The last three of my cases are especially interesting from an etiological point of view, because they were unquestionably due to the alteration produced in well-combined milk mixtures by the process of sterilization. In each instance this

had been resorted to for several months before the appearance of the symptoms of scorbutus, and these symptoms rapidly subsided upon stopping the sterilization without otherwise changing the food. In illustration, I may briefly relate the history of one of these cases:

Walter ——, the child of a clergyman living in an excellent house at Chestnut hill, one of the healthiest suburbs of Philadelphia, was seen in consultation with Dr. R. H. Bolling. The little patient's parents were perfectly healthy, as were also several older brothers and sisters. He had been fed artificially from birth, the food being a mixture of sterilized milk and water, in the proportions at first of one third, then one half, and next two thirds, with one teaspoonful of sugar and two and one half teaspoonsful of cream to each six ounce bottle. He apparently thrived well until the age of eight months, when the symptoms of scurvy appeared. They consisted of spongy and bleeding gums, pains in and immobility of the legs, and some swelling about the knee and ankle joints.

At the date of my visit, about three weeks after the beginning of the attack, the infant, while pale, was sufficiently well grown and fleshy, four teeth were well erupted, and there were absolutely no evidences of rickets. Both legs were passive, but not in the least paralytic; picking up the child or any attempt at moving or palpating the limbs caused violent screaming. There was moderate fusiform swelling at the lower third of each thigh and above the ankles; these regions were extremely tender to the touch, but the investing skin was normal in appearance. The gums in proximity to the erupted teeth were greatly swollen, overlapped the teeth, were livid purple in hue, and bled upon the lightest touch. The appetite was fair, the bowels were regular, and beyond fretfulness, restlessness, and disturbed sleep, there were no further symptoms of illness.

The treatment adopted was discontinuance of sterilization without alteration of the composition of the food, the administration of a teaspoonful of orange-juice, and the same quantity of raw beef juice three times daily, and later, small doses of citrate of iron. Result, complete recovery within three weeks.

This case simply points the moral that sterilization is not a uniformly applicable method for the preparation of milk for feeding infants, and that by impairing the digestibility of the albuminoids (Leeds) it may produce various manifestations of depressed nutrition, among them scurvy.

Attention to all the hygienic rules is necessary in the management of scorbutus in infants, but regulation of the diet is the main point in the treatment. Condensed milk, farinaceous foods, and above all, the so-called "baby foods," must be absolutely forbidden, a well-proportioned food having cow's milk for its basis being administered in proper quantities and at proper intervals. This may be pre-digested or Pasteurized if deemed advisable, but never sterilized at a high temperature. Orange juice and raw beef juice are useful additions to the diet. Olive oil rubbed into the limbs and trunk has seemed to me to relieve the tenderness and aid in nutrition, and appropriate doses of ferrated elixir of cinchona, or other bland preparation of iron, are of service during convalescence.

DR. T. M. ROTCH of Boston, in a letter, said:

The first really good work on scorbutus done in this country was by Dr. W. P. Northrup. It was he who first drew attention to these cases, and the credit should be fully awarded to him. He, Dr. Rotch, had seen several cases in the early days of his practice but did not appreciate what he was dealing with. During recent years, he had seen about twenty distinctive cases.

In a somewhat heated discussion in a Boston meeting, he recently combated the traditional views held by surgeons, and remarked that one who overlooked the existence of a distinct disease, scorbutus, in infancy and childhood, would be apt to meet with more cases of so-called acute rachitis, than one who understood the pathological conditions of the bones in scurvy. Is there such a thing as acute rachitis? Some of his scurvy cases seemed to have been superadded to an original rachitis. The scurvy rapidly recovered with orange juice, while the rachitis took the usual slow course. The proper treatment is, he be-

lieves, essentially that which Dr. Northrup has already so clearly stated. By it he has achieved some brilliant results in his more recent cases.

DR. F. FORCHHEIMER of Cincinnati, in a letter, said:

I wish to express my indebtedness to Dr. Northrup and the New York Academy of Medicine for bringing up the subject of scorbutus, so very important as an etiological factor in diseases of children. Since my attention has been directed to the subject, I am convinced that the condition exists in many more cases than we had heretofore supposed. I believe that I have seen it, even when slightly developed, producing symptoms on the part of the nervous system, the digestive tract, the blood, and general metabolism, that have been speedily relieved by anti-scorbutic measures. In obscure cases, as well as those thoroughly well developed, missionary work is necessary, and I deem it an honor to be able to assist in this direction. In reply to your questions, I would answer as follows:

1. The diagnosis rests upon the hæmorrhagic nature of the symptoms. I believe all the forms classed under the head of purpura simplex or hæmorrhagic, peliosis, and scorbutus to be the same, i. e., scurvy.

2. The hæmorrhages that are diagnostic are those which occur under the skin and in the mucous membrane. Other important symptoms are the condition of the mouth, and anæmia.

3. The hæmorrhages into the skin and peculiar condition of the mouth may exist together or alone. It is difficult to estimate the condition of the mouth at its proper value in those cases in which there are no teeth. Where teeth are present, the mouth condition may be mistaken for stomatitis ulcerosa, but the therapeutic experiment will decide the question in a very short time.

4. In my four most typical cases the food was as follows:

a. A proprietary food.

b. Adult food in a foundling asylum.

c. Bread and jelly to the absolute exclusion of everything else.

d. Cow's milk, which was never digested and was badly borne by the patient.

5. The diagnosis rests on the following symptoms: pain

in the joints and extremities, spots in the skin, and anæmia. By anæmia, I refer to the appearance of the child, including the peculiar color of the skin. Anæmia may be demonstrated by blood analysis, but it is hardly great enough to explain the peculiar muddy complexion which is often present. It is possible that some peculiar chromogenic substance may exist in this disease. In one of my patients, this peculiar complexion persisted for fully eighteen months after the patient had been apparently restored to health.

6. I believe scurvy to be a collection of symptoms, the cause or causes of which are unknown. It seems, from present knowledge, to be some chemical change, probably hæmic, which can be overcome with certainty by the administration of orange- or lemon-juice and proper food. It will be the province of future investigators to determine the primary cause.

7. The symptoms disappear very rapidly under treatment. The joint symptoms begin to improve in a few days, then the mouth, then the spots, and lastly the anæmia.

8. My most successful treatment has been orange-juice, lemon-juice, and good milk. I cannot refrain from laying stress upon the aid to diagnosis which is offered by treatment.

I have seen ten cases of scurvy, and have reported four in detail.

DR. A. D. BLACKADER of Montreal, in a letter said:

I have seen no cases of scorbutus in infants in Montreal. The only case I have seen was one shown me by Dr. Barlow while I was in London some years ago. On receipt of your letter, I went to a number of representative medical men of large clinical experience in this city, asking them if they had recognized any cases either in hospital or private practice. None of them had done so. They had either not seen scorbutus, or had failed to recognize it.

DR. WM. F. LOCKWOOD of Baltimore, in a letter, said:

I regret very much that I am unable to furnish any notes of cases of scorbutus in children. The only instance, so far as I am aware, which has come to my notice was that of two years ago, which was published in Dr. Northrup's

paper of September, 1891. Among my dispensary cases, which are mostly negroes, rachitis is very prevalent. They seldom receive any of the artificial foods, but from early infancy eat whatever the table affords. I have been greatly surprised at some of the vigorous specimens which a very mixed diet occasionally develops.

DR. W. D. BOOKER of Baltimore, in a letter, said:

I have been on the lookout for scurvy in infants ever since Dr. Northrup called attention to the subject, but I have not yet seen a case that I could diagnose as such, and Dr. Lockwood's case is the only one I have heard of in this city.

DR. J. LEWIS SMITH: The fact that so few cases of scurvy have been reported in this country, is perhaps because the symptoms of the disease were mistaken for those of rachitis. For many years I have been connected with the Out-Door Department of Bellevue Hospital; scarcely a day passes that we do not see there one or more cases of rickets, while scorbutus is comparatively rare,—very much rarer than rachitis. One reason why we fail to recognize and diagnose scorbutus in very young infants, is that the peculiar anatomical changes in the gums do not take place when the teeth are absent. In old people and in very young children, who have no teeth, we do not observe the changes in the gums, which have been so clearly and so fully pointed out by Eustace Smith, Barlow, and others, and which are of such great diagnostic value. In rachitic children, dentition is delayed, and until the teeth appear we may not be able to tell whether or not the child has scorbutus, too. I think that most of us who have been at work for many years, have made mistakes in some cases, and diagnosed rachitis without the accompanying scorbutus, since the chief cause of both is a faulty diet and the symptoms in the two diseases are often very similar. There is, in rachitis, not only the pseudo-paraplegia and tenderness on moving the child, but there is also apt to be an exuberant growth of the capillaries under the periosteum, with thickening of this membrane, feeling under the finger like pulp, and this might be mistaken for the swelling due to effusion of blood in scurvy, unless, if the case reached the autopsy table, a microscopic examination were made.

I believe I have, in times gone by, diagnosticated cases as ulcers of the gums from stomatitis or gingivitis, which, in the light of our present knowledge, we should designate scurvy. In the cases of scorbutus that I have seen, the patients usually were suffering from malnutrition. They were thin and pale, and suffered from mental depression, so far as I could make out, before the characteristic symptoms appeared. The symptoms of the disease have been so fully entered into by the previous speakers, that I shall not take the time to repeat them. The prognosis I believe to be generally good.

Of course, scorbutus is not a new affection, as its presence in seamen who went off on long voyages and who lacked a vegetable diet, had been noted and written about many years ago. The improvement by a fresh vegetable diet is, I believe, more rapid than it is in rachitis by the use of fatty food with earthy salts or minute doses of phosphorus, now so frequently prescribed by pediatrists.

DR. A. JACOBI.—I believe we are all under obligations to Dr. Northrup for having called our attention to the frequency of scorbutus. I know that since the time the doctor's paper was published I have seen quite a large number of cases—more than before. I had seen a few cases before that time, and I believe I made a correct diagnosis, but I am sure that I must have overlooked a number of them. I believe the disease occurs with particular frequency at certain times. It occurs more frequently at the very time when infectious diseases are rife. As a rule, I meet with the majority of them in the winter. In my note-book I found the records of three cases which occurred during the first half of March, 1892. Most of my cases occurred when the weather was chilly or cold and moist.

I agree with the statement made by several of the speakers that the disease is by no means confined to the poorer classes, but I have not seen a single case in a family where the children received normal food; most of the babies were artificially fed. Quite a number of them were fed with what I have always considered an abomination, namely, sterilized milk exclusively. Since the sterilization of milk was devised and has become so common, its exclusive use as a diet has often resulted in much harm.

It is true that germs are kept out by the process of sterilization, but at the expense of the chemical composition of the milk. While many babies will thrive on sterilized milk, many others will not.

It is true, as Dr. Smith said, that scorbutus is a very old disease. It has broken out in asylums and among seamen on shipboard, and has been observed in very old people. One of the most important symptoms is the spongy gums, while, as has been pointed out, absence of teeth in young babies renders the diagnosis less easy; yet there are exceptions to this rule. I have seen scurvy in one child aged four months, in one of six months, and in one of six weeks that was fed exclusively on cow's milk from birth. The child of six weeks and that of four months had no spongy gums, but that of six months, without a tooth, did have spongy gums. The diagnosis often cannot be based on the condition of the gums alone, and here the question comes up, Is scorbutus a disease by itself, or is it only one of the various forms in which the peculiar condition of the blood-vessels, or perhaps of the blood itself, manifests itself? Is it a clinical entity, or is it allied to the various forms of purpura, hæmophilia, etc.?

There are some who affirm that scorbutus ought to be the general name of this class of affections, particularly Koch, in his memoir on the subject. We really do not know what scorbutus is, or what the other diseases are which show similar manifestations. Baumgarten, in his extensive work published in 1890, does not speak of scorbutus at all in connection with bacillary infection. Thus far we are not much advanced in the bacteriological knowledge of the disease.

What did we formerly diagnose instead of scorbutus? I know that many, many years ago I diagnosed such cases as acute rickets. Now and then I have seen a diagnosis of rheumatism made in such cases. I do not believe, however, that scorbutus was diagnosed as acute articular rheumatism twenty or thirty years ago, for at that time the occurrence of the latter disease in children was almost completely denied. This very winter I saw a case of scurvy with swelling of the knee-joint, in which a diagnosis of traumatism had been made. Such a diagnosis may be possible in the early stage of the disease, when nothing but the local hæmorrhages exist. Ulcerative stomatitis

should never be mistaken for scurvy. It is a parenchymatous inflammation, resulting in speedy degeneration of the cells and breaking down of the tissues in such a way that the edges of the incisor teeth and sometimes the canines, also, are changed into a white pulpy mass. The ulceration in some cases may extend to the cheeks, resulting in a condition somewhat similar to noma.

In the treatment of scorbutus, I have employed fruit juices with good results, and insisted on animal food. Those who are acquainted with my views know that I seldom, if ever, permit the use of pure milk in the feeding of babies under a certain age. I always add barley or oatmeal water to the milk; beef juice is very beneficial in these cases. As regards drugs, I employ phosphorus, which has not been mentioned at all. The drug should be given in substance, not in the form of the salts, which are eliminated by the fæces and urine. I employ the elixir of phosphorus, found in the new U. S. Pharmacopœia, in doses of ten, twelve, or fifteen minims, t. i. d. I know of no more rapid tissue builder than phosphorus in this form.

The prognosis of scorbutus is very favorable; I have seen very few deaths resulting from the disease. In the course of a few weeks or less, such a child is restored to health under proper treatment, if there be no complication. In one case coming under my observation, that resulted fatally, the child had a very persistent diarrhœa, which the attending physician was unable to check. The patient had as many as ten or a dozen mucous discharges from the intestines daily. It may be that in such a case the diarrhœa is the cause of the scurvy. If a bacillus is the primary factor in scurvy, then this case would suggest that the ubiquitous bacillus coli communis is the one. This child died very speedily.

It appears that no healthy baby will ever be taken with scurvy, just as none such would take diphtheria. If the mucous membranes in a child's throat are in a healthy condition, the germs of diphtheria cannot locate and thrive. Before scurvy can set in there must be a diseased condition of the genital organs, or of the intestinal tract, or of the mouth or respiratory organs. Indeed, in all the cases I have seen there was an abnormal condition of the mucous membrane of the alimentary canal. In quite a number of the cases a severe bronchitis had preceded the scurvy.

DR. L. EMMETT HOLT.—There is one very interesting question that has come up in the discussion regarding the use of sterilized milk. Does sterilized milk cause scurvy? That question was asked me last year by an English physician who is writing up this subject. He said that the opinion was current in London that it was a potent factor in the causation of scurvy. At the New York Infant Asylum, it has been the custom during the past five years to sterilize the milk used for infant feeding; this has also been done for the same length of time at the Nursery and Child's Hospital, and at the Babies' Hospital, and yet I have been unable to find that more than one case of scurvy has occurred in those institutions during five years. In that case, the child was fed exclusively for two months on malted milk.

With reference to the season during which scurvy occurs, I have recently seen four cases, all in the summer. One of the most typical cases was in a child in blooming health; until the age of eight months, it had never had a sick moment. Then he became restless and seemed to feel pain when he was handled. Then ecchymoses appeared about the knees and shoulders, and there was indisposition to use the limbs. The child's food had been pasteurized milk to which a proprietary food had been added. The latter was stopped, and no other treatment instituted. The symptoms disappeared in three or four days, and have never returned.

In another case of scurvy, there was an immense swelling about the right knee, and a diagnosis of sarcoma of the knee-joint was made. In that case there were no other symptoms excepting cachexia and the spongy, bleeding gums. The child was an extreme case of malnutrition. It was about ten months old, and weighed eight pounds. Malted milk was given, and it agreed with the child so well that it was decided to continue it. After two months of this diet, this swelling developed about the right knee, and it increased until it attained the size of a child's head. The patient died, and at the autopsy it was found that there was a separation of the lower epiphysis of the femur, and immense sub-periosteal hæmorrhages. There were purpuric spots in the mouth.

In another case I have seen, a diagnosis of osteitis had been made at one of the best hospitals in this city, and the

limb was put up in plaster of Paris. In another case, the diagnosis was infantile paralysis, and the physician who tested the muscles with electricity stated that they did not respond to faradism. Recovery in this case took place within three weeks. Between scorbutus and rickets, I think it is hard to make a mistake. Rickets is chronic in its course. In scorbutus, we have symptoms coming gradually, but they are acute and severe; one of the most striking is the exquisite tenderness of the limbs. In one case, a cat rubbing its back against the limb would cause the child to scream. Even pressure from the clothing causes discomfort. In five of the six cases I have recently seen, scurvy was traced to the use of patent food. In one case, the child received an ounce and one half of the food daily for six months. Of these six cases, only one proved fatal. The symptom referred to by Dr. Taylor, namely, the pseudo-paralysis, was striking in three of the cases.

As regards treatment, it seems to me that this is a dietetic disease, and that it is curable by correcting the diet, without the aid of drugs. I depend on fresh milk and beef- and orange-juice, afterwards tonics. In most of the cases I have seen, there was a marked degree of cachexia, but in two or three this was absent. With one exception, the cases occurred in private practice, and all but one were living in the country, where I think the infant food preparations are used more extensively than in the cities.

DR. WALTER LESTER CARR.—Scorbutus is rarely observed at the institutions and dispensaries of this city. Of 2,315 children seen at the Out-Door Department of Bellevue Hospital, there was only one case of scurvy. Among one thousand patients treated during the past year at St. Mary's Free Hospital for children, there was not one with scurvy. I reported an interesting case of scorbutus in the *Medical Record*, October 8, 1892. The patient was an infant of fifteen months. She had been nursed for six weeks and was then given condensed milk and a patent food. The swollen, tender thighs, bleeding gums, and pseudo-paralysis were very marked. Recovery was rapid under anti-scorbutic diet.

I have just seen a pronounced case in private practice.

The patient was a baby of nine months. He was nursed for a few days and then given diluted milk, which was stopped at the end of four or five months. Malted milk was then used. During the summer, the baby had a severe enterocolitis with persistent vomiting. The gastro-enteric symptoms lasted for three months. Pain and swelling of the limbs, especially of the thighs, supervened, and a diagnosis of rheumatism was made. When I first saw the baby, he was lying on a pillow; he had no strength, was very much emaciated and anæmic in appearance. Both the upper and the lower extremities were motionless. The baby seemed to be suffering intense pain, and would cry when touched and even when any one approached him. The skin was dry and scaly, and there were some papular patches but no subcutaneous hæmorrhagic lesions. The head was comparatively large. The back of the head was denuded of hair; the perspiration was profuse. The thighs were very much swollen, especially the lower third of the right thigh. There was no swelling of the ankles or hips. There was extensive swelling about the radius and ulna of each forearm and tenderness over the upper end of the humerus. The lower epiphyses of forearm were enlarged. The viscera and glands of the abdomen were normal, but there was some abdominal tenderness. The lymphatics of the groin were slightly enlarged, also those at the back of the neck. There was bronchitis, but no evidence of consolidation. The gums were normal, and there were no teeth. The rectal temperature was 100° F.; pulse 122. This case was treated entirely by fresh milk, beef-juice, etc. The swellings of the forearms disappeared first, in about seven or eight days. Those of the femora were slower to yield.

It is rather remarkable that the hæmorrhages in these cases are most severe in the regions of the greatest epiphyseal growth; for instance, at the lower end of the femur where the epiphysis appears just before birth and which is the last to ossify. Tender, swollen thighs I consider to be the symptom of scorbutus most frequently present. There may be swellings in other locations, but that of the thigh is the most diagnostic.

Swollen and bleeding gums are characteristic, but these evidences of disease may not show at all if the teeth have not appeared.

Relief of the symptoms by fresh milk, fruit, and vegetables can leave no doubt as to the diagnosis of scorbutus.

DR. J. H. FRUITNIGHT.—Since the publication of Dr. Northrup's paper on this subject, five cases of the disease have come under my observation. The patients were all young infants, ranging in age from eleven to twenty months, with one exception, namely, a boy seven years old, who had lived on a diet of salt meat and bread. The children all belonged to families in moderate circumstances, who were able to feed them properly. The cause of the scurvy was the prolonged use of artificial foods. One of the children had been given four different kinds of artificial food in succession. This patient was in a condition almost approaching dissolution. It suffered from bloody diarrhœa, and had hæmorrhagic spots, the size of a silver dollar, over the body, and an extensive ecchymosis of one eye. The right ankle-joint was swollen to the size of a large orange, and was very sensitive to the touch. Under a corrected diet and fruit-juice the child recovered in about three weeks. In another case I have seen, the scurvy was superadded to rachitis.

At the St. John's Guild Hospital where sterilized milk is employed, no case of scorbutus has ever developed; in one case we had there, the disease had developed before the child was admitted to the hospital.

I regard the spongy gums as the most distinctive symptom of the disease; this, in connection with the swollen condition of the limbs and joints and the extreme tenderness, is the tripod of symptoms upon which we must depend. As regards medication, none is actually required. I rely chiefly upon a correction of the diet and orange-juice. The ecchymoses usually disappear first, then the pain, and last the swellings.

In regard to the question of scorbutus being a separate disease, I am of the opinion that it is, although belonging to that class of dyscrasia in which there also is an interference with the nutrition of the blood-vessels. It has frequently been confounded with acute arthritis and the various forms of purpura.

DR. MAX EINHORN.—I simply wish to add a few words to this discussion. I have had an opportunity of seeing one case of scurvy at the German Dispensary. I shall

not, however, go into the details of this case, but simply mention some experiments I made nine years ago with Prof. Ehrlich, of Berlin, bearing on diabetes. The experiments were made on frogs, which were placed in a watery solution, to which various substances were added. Frogs were placed in water containing bicarbonate of soda, and at the end of fifteen or twenty days there was a red discoloration of the fluid, which was proved to be due to the presence in the water of red blood-corpuscles, not simply hæmoglobin. At the autopsy of these frogs it was found that the legs were swollen. I think these experiments have some bearing on the ætiology of scorbutus. I think it possible that scurvy is due to a chemical change in the blood, perhaps relating to its increased alkalinity or the presence of an abundance of the sodium salts.

DR. S. JONES.—Dr. Holt stated that in the majority of cases he had seen, the children had been taking a patent food. He rather led us to infer that he regarded that as a positive factor in the causation of the disease. I think that most of us believe that scorbutus depends rather on malnutrition or on a deficiency of some of the elements of nutrition than upon any morbific agent. I would like to ask him whether he believes, as the result of his observation, that these various proprietary foods may possibly from fermentation or improper preparation be an active factor in the production of the disease, or whether they simply act as a negative agent as regards causation.

DR. L. E. HOLT.—There was only one case in which anything like a positive experiment could be made as regards the part played by the prepared food in the production of the disease. In that case, the child when taken sick was being fed with it, and recovered immediately when the food was discontinued. The food in that case certainly seemed to be the exciting cause. I do not suppose that in every instance it was the only exciting cause. Five out of the six cases had been on the same food.

DR. J. E. WINTERS.—One of the worst cases of scurvy I ever saw was at Smithtown, L. I., last June. The child was nine months old, and had been fed exclusively on a patent food from the day of birth. It had been under treatment for rheumatism. The child was put on sterilized milk and soon recovered without any other treatment. In

all the cases I have seen, I have employed that treatment; I have never used orange- or meat-juices. In sterilizing milk, the sterilizing process should not be continued for too long a time, not longer than fifteen or twenty minutes with the Arnold's sterilizer.

DR. NORTHRUP (closing the discussion).—The subject of this meeting is, practically, scurvy up to date. I am very desirous of collecting all the cases of the disease we have seen. We want to photograph one case on another, and see what the composite picture is like. Adding the cases of scurvy reported this evening to those previously reported in this country gives us a total of seventy-two cases, exclusive of Dr. Jacobi's, who did not state how many he had seen. This forms our contribution from this side of the water. A Leipsic observer has just reported fifty cases, including the eleven American cases already on record. I should like to have all the members who have seen cases of the disease send me the particulars, so that I can include them in a paper which shall bring together all the cases we have observed in this country.

There are two things that are maligned to-night. One is sterilized milk, and the other is a patent food. I personally do not believe that sterilized milk can cause scurvy. In the New York Foundling Asylum there are seven or eight hundred babies fed from year to year on sterilized milk, and in that institution only one case of scurvy has ever been seen, and that developed outside. As to the food: you tell me the name of the prepared food that is most advertised and most sold, and I will tell you the one that causes the most scurvy. In England, every shop-window, every railway station, every six-penny novel presents a conspicuous advertisement of a certain baby food. In one of the hospitals there, I asked a member of the staff what caused so much scurvy in London; he changed the subject, but later the house physician told me he thought the prevalence of the disease was due to this food. Wherever a favorite brand of prepared food has a large sale, there scurvy will occur.

Stated Meeting, March 1st, 1894.

POTT'S DISEASE OF THE SPINE: ITS EARLY SYMPTOMS AND DIAGNOSIS; SPINAL SUPPORTS, THEIR HISTORY AND GROWTH; TREATMENT, SURGICAL AND MEDICAL.

By A. M. PHELPS, M. D.

(This paper had not been prepared for publication at the time of going to press.—Ed.)

DISCUSSION.

Dr. L. A. Sayre.—Dr. Phelps's paper has covered this subject so fully that there is nothing left for me to say, excepting to give my entire approval of all it contains. It again illustrates the truth of the old adage, that "there is nothing new under the sun," and that the discoveries of the present age were known many years ago. The jury-mast shown by Dr. Phelps is exactly like my own. The only thing that is new is the plaster-of-Paris jacket.

I wish Dr. Phelps had drawn more attention in his paper to the importance of recognizing the symptoms of Pott's disease in its earliest stages. In most instances, the patients are brought to us with the deformity. This should not be so. The deformity only comes on as the result of a disease that has been in existence for months, and if the general practitioner would recognize the symptoms earlier, the deformity could be prevented. The difficulty of swallowing, the pain in the abdomen, and other symptoms are ascribed by the physician to indigestion, or worms, or some similar complaint, and he does not suspect Pott's disease until the deformity appears. That is not creditable to the profession. In these cases, with obscure symptoms referred to the anterior portion of the body, the child's move-

ments should be watched to see if there is an awkward gait or muscular rigidity. I think all these cases, if they were put under proper treatment before the deformity appears, would recover perfectly, without any deformity at all,—at least, the vast majority of them would. I have seen many cases of spondylitis (which term I prefer to that of Pott's disease) in which recovery occurred without any deformity.

In the treatment of these cases, it is absurd to attempt to apply a plaster-of-Paris jacket to very young children with undeveloped pelves. Such patients should be kept in a horizontal posture, either by means of the wire cuirass or this little wooden frame which Dr. Phelps has exhibited. The objection to these is that they get soiled so easily. The object is to anchor these children in a certain position, and keep them so. This is a photograph of my cuirass, as recently modified by my son. It is to be worn next to the skin, and the steel rods are covered with leather or hard rubber. It is clean, and, no matter how much the child moves, the apparatus remains firmly fixed. After the child grows older, and the pelvis is big enough, I prefer the plaster-of-Paris jacket. The wooden jacket shown by Dr. Phelps can only be manufactured by a skilled mechanic, whereas the plaster jacket can be put on by any physician, and can be changed as the growth of the child requires. I had one case under my care, an adult female suffering from paralysis, who wore a plaster jacket four years without having it changed, and it was perfectly clean when it was taken off, having been kept so by running a whalebone between the skin and the jacket, and a handkerchief wet with cologne water thus drawn up and down. Some say that plaster of Paris becomes foul and breeds vermin, but I do not believe it; I believe that the vermin were there before the jacket was put on. The plaster jacket should not be varnished. In one case in which this was done, the patient's body, after she removed her elegantly varnished jacket, was found to be covered with sores. Here are two thick cylinders made of plaster of Paris; one is varnished, the other unvarnished. Into these cylinders I will blow the smoke from this pipe. As you see, the tobacco smoke escapes freely through the unvarnished cylinder, while it is impossible for me to force it through the other. That shows the difference between

a plain plaster-of-Paris jacket and one that is covered with a coating of shellac, or any of those gum jackets which are impervious. For the same reason, the leather jacket causes excoriations, and is especially objectionable.

A very valuable addition to the plaster-of-Paris jacket in high dorsal disease is the brace devised by Dr. Whitman, which keeps the shoulders back and secures additional rest of the spine. As regards the use of the jury-mast in spinal disease, I think it should always be put on, even if the disease is very low down. If the disease is in the upper portion of the spine, it should certainly be put on. In all the cases I have seen, the patients recovered from the paralysis. I have never performed laminectomy, because there was no occasion for it; if necessary, I should not hesitate to do it. Many cases derive benefit from counter-irritation, the actual cautery, etc. I formerly thought it necessary to open all the abscesses that appear in these cases, but during the past two or three years I have seen a number of cases in which the abscesses disappeared without surgical interference, whether by absorption or otherwise, I do not know.

DR. V. P. GIBNEY.—I have not much to add to this subject. I was much interested in Dr. Phelps's paper, especially in the historical sketch he gave us.

Among some of the things in his paper that I might criticise is the statement regarding the bad prognosis in adult cases. It seems to me if Dr. Phelps will give the same attention to adults that he does to children, he will get just as brilliant results. For many years I was inclined to look on the adult cases as a *noli me tangere;* but later on, when I came to look upon the condition as one of broken back, or fracture of the spine, as we might call it, and bore in mind how long a time it required to heal a back thus fractured, I treated them with the plaster-of-Paris corset and jacket, and soon began to get very satisfactory results. I confess that now nothing gives me more pleasure than to get hold of an adult case, with or without paraplegia and with or without abscess. I do not "hanker after" the paraplegia cases, but I am not disposed to turn them away. I put on a solid plaster-of-Paris jacket and let the patient wear it for about a year, changing it once or twice during that time. If I find any one who objects to the solid jacket,

I order the corset jacket instead, and provide the patient with a swing, so that he can employ self-suspension once or twice weekly, just sufficient to lift the heels from the floor. I let him get into the swing, and after removing the corset I apply the actual cautery, if there are any points of tenderness; the corset is then replaced. I reported quite a number of these cases in the *New York Medical Journal* three or four years ago, and since then I have had others. In a number of cases immense abscesses have occurred, involving one or both iliac fossæ, and forming large tumors on the thigh, which required aspiration every two or three months. The patients are then kept quiet for twelve or twenty-four hours, during which period ice is applied over the site of aspiration, and then the patient can go about as usual. I do not inject iodoform or anything else into the cavity. These adult cases get well much sooner, in my experience, than do children. They get well in from twelve to eighteen months, and it is a rare thing to see a relapse.

We never like to talk about abscesses in the Orthopœdic Section, because we usually get into a hopeless wrangle about them; but I was pleased to hear Dr. Sayre say that he no longer opens every abscess he sees. I believe Dr. Phelps's impression that adult cases do badly was derived from the fact that the cases he has seen were in the hands of the general surgeon, who cuts every abscess he sees.

DR. A. B. JUDSON.—I agree with Dr. Phelps in holding the head back instead of trying to raise it, but for this purpose I much prefer the Taylor chin-piece to the jury-mast. The brace is a lever making pressure on the projection and counter-pressure below at the pelvis and above at the shoulder or chin. Antero-posterior pressure thus made can be applied continuously day and night, while suspension is intermittent. Suspension is simply a form of traction. In straightening a crooked nail we do not apply traction, but lateral force or leverage, just as we support a crooked back. I have no word of criticism regarding the use of plaster-of-Paris in Pott's disease. There are thousands of patients all over the world who would have no mechanical treatment at all if they did not have, thanks to Dr. Sayre, the plaster-of-Paris jacket, which, properly applied, is vastly better than no mechani-

cal support. In regard to the steel brace, I have this to say: It is light and clean; it leaves the muscles of the trunk generally at liberty; by the use of soft steel and the adjustment of straps and buckles, the pressure may be regulated to a nicety, the pressure may be applied to that part of the projection, the upper or the lower part, as may be desired, and the brace may be readily and conveniently adjusted to the growth and changing shape. In its application, the skin covering the projection should be watched, and pressure gradually increased until the tolerance of the integument is reached. Beyond this it is impossible to push mechanical support. The lobster and crab have the skeleton on the outside, but in our efforts to supplement the defective human skeleton by an external one of steel, we are baffled by the intervention of the vulnerable skin. The steel brace enables us, while recognizing our limitations, to safely and comfortably carry mechanical treatment to its utmost limit; its disadvantage is that it can be used with satisfaction only by residence in or frequent resort to places where skilled workmen and special practitioners are found.

DR. W. R. TOWNSEND.—In most of the points referred to by Dr. Phelps I agree with him, but in some I differ. I understood him to say that he would operate in cases of paralysis, if there was no recovery by mechanical treatment inside of two months. I think that is too early to recommend operation. In the majority of cases, ninety per cent. or more, the paralysis lasts longer than two months. In one case I saw, it lasted two years under what we considered proper treatment.

As regards the opening of abscesses in these cases, if Dr. Phelps excludes the so-called cold abscesses, and simply operates on those containing pus, then we both agree. In my opinion, the ordinary cold abscess which does not contain pus germs, should not be opened.

I believe that the plaster-of-Paris jacket is the best material to use in the treatment of these cases, especially for adults, and that young children should be placed in a frame or bed, as Dr. Phelps suggested.

DR. R. H. SAYRE.—In the main, Dr. Phelps has treated this subject in a most admirable manner, and there are only a few points on which I differ from him.

As regards the diagnosis of Pott's disease, I think Dr. Phelps has laid too much stress upon the prevalence of the rigid spine. Wherever we have inflammation of a joint, we have rigidity of the muscles controlling that joint, but in the early stages of the disease the muscular rigidity is not apt to be very marked, and a man who has not seen many cases is likely to overlook them before the kyphosis makes its appearance, if he looks for a rigid spine, the rigidity being limited to a very small area of the spine; the patient complains of vague pains here and there, and the spine is not so very rigid, as Dr. Phelps's remarks would lead one to expect.

In adult cases I would take middle ground between Dr. Phelps and Dr. Gibney; the former took a very dubious view of them, while the latter said they would get well in twelve months. I have seen adult cases which lasted many months, certainly longer than twelve, and which recovered completely. I would say they required treatment as long as children. The reason they are not so apt to recover as children is that they do not get the same care and attention.

In regard to the treatment, it seems to me that the location of the disease in the spine has a certain amount of bearing on the mode of treatment to be adopted. For disease involving the lower part of the spine, for instance, I think an apparatus which envelops the body is better than an antero-posterior support, and for the purpose of enveloping the body I think the plaster-of-Paris jacket is the best thing to use. In disease involving the upper portion of the spine, it seems to me that better control of the parts is gained by means of posterior steel supports, with the attachment of Dr. Whitman, making pressure on the tips of the shoulders instead of in the axillæ. If you make your pressure backwards by means of bands in the axillæ, you do not get such efficient pressure as you do by pressing on the heads of the humeri. The plaster jacket is not as suitable for these cases as it is for cases in which the disease is lower down. In the cervical region the plaster jacket is only useful as a means of support for the jury-mast. The use of the jury-mast is not alone to make pressure backwards; it should also make traction upwards. The shape of the jaw piece should be a little different

from that shown by Dr. Phelps. If the jaw piece turns down, backward, so as to avoid chafing the throat, traction is not comfortable. I think traction above the head is preferable to traction by means of something going back of the head and under the chin. If the patient can drop his jaw against this spur under the chin, his head is not lifted off his spine, while if he cannot open his jaws, he is in a very uncomfortable position, and practically I find more comfort and greater ease of application in a jury-mast which goes above the head. This is the cuirass which I apply to children who are too young for the plaster jacket, and which I believe to be superior to the ordinary cuirass because it is applied directly to the child's skin and does not require to be removed when the child is dressed, and in hot weather is much cooler. It is better than traction combined with rest in bed, on account of the greater ease with which the child can be taken from place to place and given fresh air.

DR. A. M. PHELPS (closing the discussion).—Dr. Gibney stated that he does not have bad results in adult cases. I do, and I have seen bad results from others, not alone from the general surgeon, but also from orthopœdists. The adult cases do not do as well as children; I think that is the observation of every man who has had extensive experience in this field of work. I do not mean to say they all die, by any means, but the prognosis is not so good as in children.

In regard to the antero-posterior brace referred to by Dr. Judson: In from thirty to forty per cent. of the cases of Pott's disease, there is lateral curvature, and the antero-posterior splint cannot be applied effectively when lateral curvature is present.

I do not think it is necessary to adjust the jury-mast in disease of the spine below the third or fourth dorsal vertebra, if you apply a snug plaster corset properly adjusted and fitted. The rigid chin-piece of Dr. Taylor I do not use because the jury-mast and sling are more effective. As regards paralysis or atrophy of the muscles in consequence of bracing, I think that is fallacious. I do not think either the plaster or aluminum jackets will produce atrophy of the muscles. If they did in Pott's it is of no importance because development takes place rapidly after

the corsets are discontinued. I do not believe the degree of pressure can be so well regulated with the steel braces as with the plaster-of-Paris, which can be molded at will; and then the nurse or mother will remove it at will and do more harm than can be remedied by months of treatment. In cases of paralysis, I would operate within two months if incontinence of urine and fæces exists; I would not operate within that time if there is only paralysis of motion. As to abscesses, I believe the sooner they are cleaned out, the better. As to the so-called cold abscesses, I usually find that they are the hot ones. The rigidity of the spine is, I believe, always present in Pott's disease.

Stated Meeting, March 15th, 1894.

GREEK AS THE INTERNATIONAL LANGUAGE OF PHYSICIANS AND SCHOLARS IN GENERAL.

By ACHILLES ROSE, M. D.,
NEW YORK.

All those who attend the International Medical Congresses notice an unpleasant circumstance, which becomes more and more marked with every succeeding assemblage. It is the inconvenience caused by the want of one language understood by all. There are some members who understand and fluently speak the official languages; they can easily take part in every debate, no matter which official language is used by the speakers. But few such members can be found; the majority of the participants, and among them frequently some who are most prominent in their specialties, understand but one language, and thus lose about two thirds of everything spoken during the meeting. They are often unable to enter upon the discussion of a question, because they cannot understand the subject mentioned; and if they speak on some subject, as a rule it is not understood by at least two thirds of the participants in the congress.

An illustration of the difficulty thus presenting itself on account of the polyglot condition of these medical assemblies, is found in a letter written by a prominent German surgeon, dated December 28, 1892, to the president of the American Surgical Association, concerning the Pan-American Medical Congress.

The languages of the congress were the Spanish,

French, Portuguese, and English. The German was excluded, probably because it is nowhere in America recognized as official.

The surgeon says in his letter: "I do not believe that the physicians of Germany will be able to take an active part in the transactions of this Pan-American Congress, unless they are enabled to use the German language in delivering their lectures."

The difficulty in this case was overcome by changing the statutes, by allowing papers to be read in any language, provided that the authors of papers in other than the official languages transmitted to the general secretary a synopsis of not more than six hundred words, before a certain date in advance of the date of the congress. A further condition was that a manuscript of each paper of this kind was to be delivered, before or during the session, to the recording secretary of that section before which it was to be read. Remarks on articles read could be made in any language, provided the member making such remarks handed them in before the close of the meeting, written in one of the official languages. I enumerate these details in order to illustrate how complicated the difficulties of a polyglot congress are. Everybody can complete this chapter either from personal experience or by meditation.

One might think the remedy in this dilemma would be the adoption of a universal language, and indeed, this idea has already for a long time occupied the minds of the greatest thinkers, above all, of Leibnitz. His attempt was based on the supposition that every act of thinking might successfully be reduced to an arithmetical basis, if it were possible to discover symbols for the most simple comprehensions and for the combination, as well, of such symbols, as, for instance, is done in mathematical science. Already in his youth he aimed at this purpose in a well-developed plan, maintained up to old age, of a

"*Characteristica universalis*," or "*ars signum et lingua philosophica.*" However, this plan was never realized. As far as his idea was correct, it has been carried out by the signs of the mathematical and chemical sciences. A world language, so far, exists only in the telegraphic marine code.

As the attempts of Leibnitz failed in the seventeenth century, so also have failed those of the eighteenth century.

During the nineteenth century, the means of communication increased in gigantic proportion; international commerce became of far more importance than ever before; and the attempts at creating a world language have been resumed. The best known of these is the Volapük of the Rev. Mr. Schleyer, and the partial success obtained for some time by this artificial language proves the existence of a great desire for an international means of communication.

Whatever may have been the object of Volapük, it could never have been the intention of the inventor, nor could it have been expected of him, to make it an international language for scientific purposes.

It was an idea of King Maximilian of Bavaria, to transmit to history a reminder of his reign. He instructed the architects of Germany to design a new style to be named after him. Such a style of Maximilianesque was created. I have seen, in Munich, houses built after this plan. An architect—it was Semper, if I be not mistaken—when asked to take a part in this creation of the so-called Maximilian style, answered, that such a thing could not be made to order, that a style of building is the consequence of the history, the culture, life, and doings of a great period of a people. If such be the case with a style of architecture, how much more must it be the case in regard to language!

The history of this style of Maximilian's is, that it has

no history. This short history is also that of the attempts to create a new world language.

While a universal language, sufficient to satisfy the intellectual want of every people and of every time, can be as little imagined as the equality of all mankind, still such an uniformity is possible in a restricted part of human society, viz., in that aristocracy formed by art and science. It is not the masses who need such a universal language, but the men of science.

Since Latin is no more used as an international scientific language, the want of such a language makes itself more and more felt as science extends. I do not know if any, and what serious attempts have been made in regard to this desideratum. I read that the American Philosophical Society had proposed that the question of the creation or adoption of an international scientific language should be considered at a congress which was to be held in Paris in connection with the last Exhibition. I read further that the Société de Médecine Pratique had taken up the question, and a commission, consisting of representatives of the principal scientific associations in Paris, had been appointed to study the matter. As far as I have learned, these associations set their face absolutely against Volapük.

Just at present, there is much agitation in France for reform of instruction and examination in medicine. The Ministry of Instruction propounded quite recently questions in this direction, to be decided upon by the medical faculty of Paris. A commission of five professors and the rector of the faculty have considered these questions, the principal of which was whether the study of the classical languages should be abandoned. The commission in its answer said: The physician is obliged to use a terminology which is derived from the Greek and the Latin. Although he may, without having been instructed in the classics, in the course of time acquire a superficial

knowledge of the expressions, still there will remain in such a case a sentiment of inferiority because he does not know their origin. In the interest of the dignity of the profession, this sentiment should be spared to the future physician. The commission further said it would be absolutely necessary that, in addition to the knowledge of the classical idioms, the knowledge of one of the modern languages should be required, namely, the German. In the present condition of the medical science, which derives its elements from all parts of the world, every physician ought to be somewhat of a polyglot.

The rivalry of the nations is against the employment—as an international language—of one of those principally spoken in the civilized world, such as English, French, or German. In addition, these languages are insufficient for the expression of new ideas and for the composition of words. Even as it is now, the English, French, and German scholars have one thing in common: they borrow from one and the same language when new words have to be formed for new things. They borrow from the Greek, from that language which has many claims to be preferred to every other in the selection of a universal language for scholars.

In choosing the Greek, no mutual rivalry need be taken into consideration. It is the old, old idiom of a small nation and of a small country. The language is rich and musical, clear, and precise, and especially abounding in combinations. It is able to render every modern idea completely, and already it has, in this regard, given life to thousands of words. In thousands of schools, and in every university, it forms a necessary part of instruction. Not only do we use a multitude of Greek words in our daily intercourse, but our entire medical terminology, also the general terminology of the arts and of the sciences, is, for the most part, dominated by the Greek language.

The magnificent structures of the ancient Greeks, their equally splendid works of sculpture, have been so little approached by us, that nobody in the whole world would entertain the possibility of a comparison in our favor when modern achievements are contrasted with the masterpieces of Greek art. The temple of the Olympian Jupiter, the Acropolis of Athens, the Venus of Melos, the Hermes of Praxiteles, are proofs that the Greeks had a much better developed sense of beauty than any other people of a later age.

Greek art is still alive, for it affords the highest examples for our architects and sculptors. Everybody knows this to be a fact.

The Greek language still lives, the same old Greek which is taught in our schools—taught, however, by the eye only. It is spoken by seven million people, and it is more beautiful and noble than any other language, just as Greek art is more beautiful and more noble than any other. There are, however, but few who seem to be aware of this fact.

Greek has once before now been the world's language. Its use was extended over a larger territory than the Latin. "*Græca leguntur in omnibus fere gentibus. Latina suis finibus*," says Cicero. "*La langue greque deviendrait la langue universelle*," Voltaire wrote. The humanists at the end of the Middle Ages thus caused its Renaissance.

Let us hope that a second Renaissance and a brilliant period of the study of the Greek language will ensue, the final purpose of which can only be the greatest possible extension of genuine science and culture.

The colleges have sprung from the Latin schools of the Mediæval Age; they have, on this account, inherited a steady preference for that language. The general use of the Latin on the part of the learned profession, has,

in the easiest manner, facilitated the learned intercourse of all. This has now, however, altogether changed. The national languages have obtained their natural rights, and should always maintain them, even if a universal or international language for scholars shall have been adopted. We must concede that it is impossible to reinstate the old relation the Latin has held—when all the lectures on any subject whatever at the universities were delivered in Latin. Neither would such be desirable.

Virchow says, in his inaugural address as rector of the Berlin University: "It was from the beginning a weak side of humanistic educational institutions to favor the Latin language. It must be conceded that they could not do otherwise. They found the Latin the universal language of church and law. They were all Latin schools. They only continued what had become a general practice in consequence of the habit transmitted for a thousand years. But for this reason, they had accepted an element of weakness. For the classical writers of Rome were in their works away behind the Greek authors. Indeed, the best among them are indebted to their Greek antecedents for their education. The school of Athens formed the background of all learned activity. Our own western civilization has adopted from the Greek literature the really moving thoughts and the facile forms. Homer, Aristotle, and Plato have continued to be, up to our time, the teachers of mankind.

"Since the Greek authors have again been read in the original, the active interest in the Latin language has been reduced. Still, the Latin remained the principal object of information. But it steadily accomplished less. As the use of the language as such became gradually less, rhetoric was omitted, restricting the study more and more to the grammar. Indeed, instruction in grammar gradually so overpowered everything that even the Latin essay became a *pium desiderium*."

The Latin, as an international and scientific language, loses every day more of its importance. Indeed, it might almost be said that it is only kept alive in purely philological and theological literature. Latin is a dead and restricted language, insufficient for the present time.

The number of Latin scientific terms, with the exception of the vocabulary for law matters, is inferior to the number of words from the Greek. Moreover, we possess only the written Latin language; the language of daily commerce has not been transmitted. The Latin of the church, of the learned, is an artificial, a forced language. It can easily be understood why, under these circumstances, the instruction in Latin became more and more purely grammatical; but why the Greek, a living language, a language just as living as our own, has been treated alike in our schools, is a question which should be addressed to all the learned world, in order to expose a wrong that has been committed and kept up for centuries.

"Grammatical schooling," says Virchow, "is not that auxiliary means of progressive development which is needed by our youth. It does not cause that desire for learning which is a presupposition of independent further development; but, on the contrary, it is manifest that many scholars, as well as their parents, regard it with hatred."

Prof. John Williams White, of Harvard college, says: "High grammar, philological research concerning forms and laws of construction, should be undertaken by no one until he is well on his course, and, it may well be, by the majority of men never at all. The study of the classics is an effective means of mental discipline, but theoretical grammar does not furnish the best field for its exercise."

Study, like almost everything else in our times, and especially in this our country, must be done at high pressure; and no time is to be lost, since many things have to

be learned. It is true the Boston Latin School does not do what it did forty years ago—teach boys for a whole year the forms, rules, and exceptions of Latin grammar without even a single sentence of illustration; the "Method of Classical Study," by Dr. Taylor, of Andover, in which he asks seventy-six questions upon the first three lines of Xenophon's "Anabasis," and one hundred and twenty-seven upon the first three verses of "Æneid," I suppose is not in use any more; yet radical change of instruction in the classical languages, especially in Greek, is needed, whether we consider either of these languages as an international medium or simply as a means of mental discipline.

In order to command a language, it is, above all, necessary to know how the people speak. The every-day language must be familiar to us. Whoever knows the conversational language of a nation has the key to the understanding of its writings like the people themselves.

The Attic boy needed for reading the Greek poets, the Attic farmer for the theatre or a public meeting, only the knowledge of the Attic conversational language in its most simple form. It enabled them to understand the tragedies of Sophocles and the speeches of Pericles.

It has often been claimed that there are remarkably few words and sentences which suffice for the common man in speaking his native language, and which enable him to understand even that which to him is a new formation. The every-day language must first be known before acquiring the art language.

Macaulay and others recommend, while learning a language, to lay aside the grammar, as the laws of speech will be easily comprehended while reading good authors. It seems to me that whosoever begins the study of a language with the learning of its rules, will never learn the

language, unless he abandons the study of the grammar and commences anew.

So long as Greek is taught in the schools according to the present methods, it will be considered as a language too difficult to be learned, and could not be selected for a universal language.

Greek is a living language and must be treated as such. It is difficult to find a proper expression without using strong terms, to characterize the erroneous common opinion that Greek is a dead language!

We frequently meet with people who, having attained a certain degree of education, make this mistake, while as a matter of fact, Greek newspapers are continually published, and new books, also, appear regularly in the Greek language, treating of various subjects. An uneducated man may be excused for such mistakes, as certainly professional philologists have contributed not a little to the propagation of such views.

The professors of the classical languages simply pay no attention to the living Greek, without having even the least semblance of any grounds for such disregard; and yet they pronounce the language of the Muses according to the usage of their respective countries, in the English, Dutch, or German manner. The pronunciation, which ought to be alone the rule, is unknown to them, nor do they wish to know it.

Nothing is easier than the proof that the Greek is not a dead language. The daily Greek newspapers published at the present time prove that the Greek language of today is still the same Greek of the classical age, showing merely some differences as each living language undergoes in the course of time. Look, for instance, into the Ἡμέρα, published in Trieste. Whoever has been instructed merely at school, on beholding for the first time this paper, will be agreeably surprised to find that he is able to understand its contents without any difficulty.

A better and more convincing proof can hardly be imagined.

The fact that the Greek language alone has preserved itself almost unchanged through thousands of years in its original beauty is, in my opinion, as a modern Greek writer expresses himself:

> "*διότι τὸ ὡραῖον εἶναι ὡσὰν λάμψις τοῦ ἡλίου ἐπὶ τῆς γῆς, διότι τὸ ὡραῖον ζῇ αἰωνίως.*"

The Greek language has been transmitted together with its pronunciation. The majority of the Greek people, kept in bondage since the Mediæval Age until 1822, were altogether unable either to read or write.

Much has been said garrulously about the degenerated descendants of Pericles, Socrates, and Phidias. Still, these degenerated descendants have the undeniable fortune to speak a language which Pericles, Socrates, and Phidias would have understood. An unbroken chain continues from generation to generation, and back again, from the Greeks of the nineteenth century to those of Pericles, Socrates, and Phidias.

The Greek language is immortal!

An interruption of Greek culture, and of the use of the pure, fine, literary language, has never happened in Greece, not even after the Latin conquest in 1204 and the Turkish conquest in 1453. This tenacity of the Greek language has been accounted for by the fact of the beauty and the magnitude of its classical literature. A large tree, which excludes all sun rays underneath its mighty foliage, will not permit other plants to be lighted and warmed, and to thrive within its reach.

The Greeks always regarded their language as of wonderful beauty, and looked upon new elements which might have been introduced into it as vulgar corruptions. School, church, administration, military, legislature,

courts, correspondents, literati of all centuries, used the pure literary Greek, and never even the κοινή, the vulgar or people's language. Even the Roman reign, which forced many nations to adopt the Latin language, had no powers to interfere with the continuation of the use of the Greek. To emphasize it once more: The Attic, the classical language, with its forms, words, constructions, expressions, orthography, has always been regarded as sacred, as something which alone has a right of existence, which had to be preserved as free as possible from new elements.

When Greece had regained her liberty, after almost four centuries of Turkish bondage, a regular government was to be erected. Countless numbers of demands were made on the language. A new life, a culture of which there had been no idea before, appeared suddenly before the Greeks. The language had to keep pace with the many new political, scientific, technical, commercial, journalistic, requirements. Another nation would certainly, under such circumstances, simply have adopted with the foreign ideas also the words of foreign people, and would have formed a half French and half hybrid language. Not so the Greeks. Their history has led them to exclude foreign words, led them to take the necessary elements from the old Greek to create new symbols for new ideas. This was a gigantic work. Stephanos Kumanudes has enumerated thirty thousand words which have been created during the last one hundred years. Let us illustrate how the work was done by a few examples: During the last century the foreign word σταμπαρία had been used for printing establishment—then τυπογραφεῖον had been formed, and from the latter a great number of combinations were made which could not possibly have been formed from σταμπαρία. In the same manner was said ἀβουκάτος, then the genuine Greek δικηγόρος, or first πόστα, then ταχυδρομεῖον, etc. These and thousands of other foreign words are now entirely out of use and may be known only to the

oldest people; the majority of the Greeks have no recollection of them.

It is a customary assertion that the modern Greek is a barbarous mixture of a good deal of Slavonic, Albanese, Turkish, and Italian, and of a little corrupt Greek. As we have seen, this is just as untrue as the assertion that the Greek is a defunct language. Naturally, such incorrect views are held among the ignorant. However, as I know from experience, such ignorance is found also among the otherwise educated classes who have studied the Greek language while at college. It is remarkable how the very Greek language, from which every other European language has drawn so freely, has been calumniated in such a manner.

Aside from the Greek as published in newspapers and books, which some are pleased to designate as an artificial old Greek in a new Greek garb, the living and really spoken language of both the higher and the lower classes, of the inhabitants of the cities as well as of the peasantry, is by no means a barbarian mixture, but rather a genuine Greek. Everybody acquainted with this language is aware of this fact. I cite as witness thereof: Ernst Curtius, a first-class expert in both forms of the Greek language, who says in his work "The New Greek and its Meaning with Regard to the Old Greek," that, excepting a few tracts at the border of the territory where Greek is spoken (as, for instance, the Ionian Isles), "even the lowest Greek uses a pure Greek language."

The question of the physical descent of the new Greeks, which cannot be separated from that of the language, is best settled by answering that of the descent of the language. According to late researches, a Slavonic descent of the Greeks can no longer be maintained. Proof can be furnished that not only are the modern Greeks not Slavonic, but also that no trace of a Slavonic influence can be found, with one exception to be mentioned presently.

A colleague, who had studied Greek and was also a college graduate, claimed, while conversing with me, that the modern Greek and Slavonic languages were very much intermingled. A Greek gentleman, a scholar, on hearing this reproach, replied: "I shall be very much obliged to this gentleman if he will mention even one single Slavonic word found in the modern Greek language." The Slavonic is entirely restricted to the designations of habitations, hills, landscapes, waters, and even then it appears only in occasional places and by no means in all Greece.

In spite of a long-continued intercourse, the Albanese have, if possible, left still less traces in the Greek language.

It is somewhat different concerning the Turkish language. The Turkish dominion was for centuries very effective and oppressive; it cannot, therefore, seem strange if words of the official language have permeated the language of the conquered people. In the written language, however, nearly everything of foreign origin has been carefully avoided.

It is true the works of the modern Greek writer are not of as much beauty as the works of the classical period, but the language is not to be accused for this. The marble of Pentelicon is not at fault when, in later periods, no Venus of Melos, no Hermes of Praxiteles, could be formed of it.

In comparing the new with the old Greek, the mistake is frequently made of considering as old Greek the written Greek of the Attic poets and writers of the classical period. The new Greek of the people's language is compared with a dialect of the old Greek written language. Unlike is compared with unlike, in order to prove that in the real living language the forms of the old Greek have been partially lost, partially changed.

The pronunciation, as taught in our schools, differs to

an extraordinary degree from that of the language of the Greek nation of the present time. This school pronunciation lacks every scientific authority. The ignorance of the new Greek shown by most of those who take sides against the modern Greek pronunciation of the classic Greek language is astonishing. Philologists have written in favor of the school pronunciation and against the pronunciation of the living Greeks, although they have never heard real living Greek spoken by a Greek.

The Greek of the schools is looked upon as a dead language; the method of teaching, as well as the purpose for which it is taught, is of no account for practical life. Those leaving school, except such as choose philology as a profession, forget what they have learned more rapidly than they have learned it, and thus it seems to be of no consequence to the teachers whether the Greek is pronounced in one way or another. A custom handed down for three hundred and sixty-five years is followed, and thus the necessity is removed of imparting to the language the sound of a living, undoubted Greek idiom.

The French, English, and Russian pedagogues think in the same manner as the German philologists, therefore the Greek language is learned in the respective countries according to the modern high German, French, English, and Russian pronunciation, and forgotten again. The fate of the Greek language in the schools seems therefore to be sealed, unless a better mode of instruction is introduced. A language which is spoken by seven million people is forcibly reduced to a defunct language.

A school which is proud of its scientific teachers should teach nothing that has been proved, and been also admitted, to be unscientific and false. Neither should this be done even with a really defunct language. Nor does it ever happen in regard to any dead or living language, except in the case of the Greek. Instruction in the living languages is not given with an invented pronunciation, and even in

teaching dead Latin and Hebrew a pronunciation is taught in the schools, which is pre-eminently based upon the living tradition. Latin is taught as it is transmitted through its pronunciation in Italy, and through the pronunciation of the Italian language; Hebrew, as it is really spoken by the Portuguese Jews.

Only with the Greek an exception is made by the school, and just in this case the existence of a living Greek language ought to be a reminder to place instruction in close relation to life, so that the scholar might later employ it for practical purposes. The phrase ought to be borne in mind: *Non scholæ, sed vitæ discimus.*

It is certainly very discouraging to the scholar who, having devoted years to the study of the language, finds that, thanks to the college pronunciation, he must pass among the Greeks in their beautiful country like a deaf and dumb person, neither understanding nor understood.

The time has passed long since when a creative activity in Attic philology and archæology was, almost exclusively, evinced in the dust of domestic libraries with facsimile and picture book. The number of archæologists, especially since Schliemann, who try to enlarge the knowledge of old Greece in the new Greece is steadily increasing. There are inducements enough, even without the idea of making Greek an international language, to employ the pronunciation of the now living Greeks. No probability exists that the ancient Greeks spoke like the college professors; certain is it, however, that their pronunciation was similar to that of the Greeks of to-day.

The pronunciation of the colleges is an invention of Desiderius Erasmus of Rotterdam. He has described the invention in his book "*De recta latini graecique sermonis pronunciatione dialogus.*" The book was published in 1528.

Which pronunciation had prevailed in Europe up to the invention of Erasmus of Rotterdam? The Greek, the same

in all European countries, learned from the Greeks. After the conquest of Constantinople, Greek scholars fled to the Western European countries, and there gave lessons in the language of their ancestors; pupils did not doubt that the pronunciation of these modern Greeks had a historical right. Reuchlin, the great German philologist of the time of the Renaissance, has learned and taught the Greek language with the pronunciation of his Greek contemporaries, hence all persons who follow his example are called Reuchlinians. The modern Greek pronunciation, although not invented by Reuchlin, is called, curiously, Reuchlin's method, in opposition to the method invented by Erasmus.

Erasmus always spoke the Reuchlinian, but never in his life according to the method called after him. He also taught his students the pronunciation according to Reuchlin's method. Indeed, he had requested his Grecian friend Laskaris to furnish him a Greek teacher, in order that his own children should learn the correct pronunciation with their own ears and mouths.

The above-mentioned dialogue, *de recta pronunciatione*, is indebted, according to a well-authenticated tradition, to the following farce: Erasmus, who was childishly vain about his Latin and Greek knowledge, and who styled himself "the most amiable prince of science," met with the following mishap: A gay visitor from Paris, inclined to perpetrate all kinds of roguery, told him the following fib: He had made the acquaintance of Greeks, very erudite men, who spoke in a manner entirely different from that in which all the world pronounced the Greek. And then he showed Erasmus how these remarkable Greeks spoke: exactly as if Greek were Dutch.

Whether Erasmus put full or only partial faith in this venture, it is certain he wanted to use it in order to aid his vanity; he intended to pose before the learned world as the inventor of this latest bit of wisdom. The dialogue was composed in the year 1528. Most of the contestants in the

battle about the Greek pronunciation have not even read this manifesto. It assumes the shape of a dialogue between a bear and a lion; is exceedingly tedious, a feeble and attenuated waltz in trivial Latin.

The bear imparted to the lion that the old Greeks might have possessed the Dutch pronunciation interspersed here and there with specimens of the French expression. This nonsense spread like a prairie fire. A flood of pamphlets agreeing with this dialogue was the result.

The invention of the pronunciation, according to Erasmus, happened in no other manner. The professors of Greek need not boast of the history of their pronunciation of the Greek language; it shows anything but a scientific basis. If any one doubts the truth of this, let him read the document of the dialogue referred to; it can still be obtained. It is hardly necessary to marvel at this story. The dialogue was published at a time when, in spite of profound erudition, there was hardly an inkling of the internal substance of a language, or even of the true relation of the languages to each other, to be found in the world of learning; at a time when the greatest imaginable nonsense was uttered in an erudite manner. Specimens thereof are the Latin essays, comprising large volumes, concerning the question whether Adam was created with a navel. At a large waste of learnedness it was also proven at that period, that Adam and Eve, before biting into the apple in Paradise, must have spoken Dutch, but after the fall, the French language.

Since the sixteenth century, a Babylonian perplexity prevails concerning the Greek pronunciation. Nobody understands the Greek of a foreigner, still less that of a Greek.

It is certainly remarkable that the perfectly arbitrary, senseless invention of a cranky pedant has remained in force since 1528. Every attempt to justify it, composing since 1528 a bulky literature, is based upon sophistry.

The followers of Erasmus have taken pains, up to the

present day, to invent the scientific proofs for the correctness of their pronunciation.

In the course of time, the pronunciation of every language changes, in correspondence to the changes of the language itself. This we can perceive in an instance quite near us: The Canadian Frenchmen, having remained in an isolated condition, speak the French language to-day in exactly the same manner as it was written and spoken in France a century ago. Dialects undergo the least changes, as may be seen by comparing the writings of Zwingli, which are written in the very genuine Zurich dialect as it is spoken to-day. Examining the Manesian manuscript, containing the songs of Zurich poems of the thirteenth century, we find the same language which the people of Zurich speak to-day in their own dialect.

The application of these examples to Greek is near at hand: It is not probable that the pronunciation changed more during the same space of time than the language itself. The classic Greek was a well-sounding language; we know this through the Greeks themselves, as well as through the Romans. The Greek of to-day still belongs to the most melodious languages; its pronunciation gives a beautiful and pure harmony.

When we consider the absurdity of the school pronunciation of the Greek, we must regret that a clumsy joke perpetrated upon Erasmus of Rotterdam—a joke which certainly does not become science on account of its venerable age—is still taken seriously by many.

I said above: "In order to command a language, it is above all necessary to know how the people speak. The every-day language must be familiar to us."

"Whoever knows the conversational language of a nation has the key to the understanding of its writings like the people themselves."

"The every-day language must first be known before acquiring the art language."

Should we not feel sorry for the student who begins learning English by studying the poetical works of Chaucer? In what a roundabout way would he finally be enabled to understand the peculiarity of the language of Longfellow; how long would it be before he would be able to derive any sort of enjoyment from this poet's writings, if he were to learn the English language by reading Longfellow's works exclusively, and, in learning it, were obliged to parse every word?

The color of a language and the kind of style of a literary work can only be fully perceived by one who is able to judge how far this language differs from commonplace daily conversation. We do not subject good wine to a chemical analysis by means of acids and salts in order to prove its value, neither do we grammatically analyze a poem to enjoy its charm.

In learning a language, we notice one thing: In order to advance rapidly we have to read, in the beginning, only such books as are written in an easily comprehensible style, the contents to be also of an entertaining character. If we choose the more difficult, serious, or tedious books, we shall not advance, but rather retrograde. If we begin with children's stories or literature for the common everyday people, we shall be surprised to find how soon we can dispense with the use of a dictionary. We soon guess and learn new words by reading the context. We thus learn to think in the language, and the more we progress, the more we enjoy the better, the higher, the more serious, the classical literature.

Professor John Williams White, of Harvard College, in a series of articles published in the *New England Journal of Education*, in 1878, entitled "Latin and Greek at Sight," recommends the instruction in the classical languages after the manner of teaching German and French, *i. e.*, to accustom the pupils to read at sight, without any preceding preparation. He mentions that the pupils

learn much more quickly and better to read the German than the Latin languages, although twice as much time is spent in the study of the latter. Concerning Greek, he says: "It is to be reckoned that it is more difficult to learn to read Greek than, for instance, German; but then there is not so much difference between the two languages as to justify the fact that pupils, after studying Greek for years, are not yet able to read without the aid of a dictionary, or through some other means of assistance, while they learn in a much shorter time to read German fluently."

In order to obviate this evil, he recommends, among other means, that the pupils should not study higher Greek or Latin grammar until they are enabled to read these languages with a certain ease, and also have read a good deal. His claims are rather modest. He says: "The study of grammar should be rendered more practical, especially during the first years. The pupil, after having studied both the Greek and Latin languages for three or four years, should be able to read the Greek writings of Xenophon, Lysias, and Herodotus, and the Latin of Cæsar and Cicero, without either previous preparation or the use of a dictionary."

Professor White, in his suggestions regarding information of the instruction in Greek, has not gone far enough, because he, like other college professors, ignores modern Greek. The literary Greek of to-day is identical with the Attic dialect in orthography, almost also in form; the syntax is here and there circumscribed and simplified. There is more difference between the Greek of Herodotus and the Greek of Xenophon than there is between the Greek of the latter and the Greek of to-day. There is more difference between the English of Chaucer and the English of to-day than there is between old and new Greek. The living, the Greek as it is spoken and written in Greece to-day, is the one which should be taught in

our schools. The Greek as it is taught in general in our schools, is simply a skeleton without life. Our college professors should not look upon Greek as a dead language, and above all they should give up pronouncing it in their barbarous arbitrary manner.

It appears to me that Greek, taught like other living languages, by one or the other modern methods—Meisterschaft's, or any similar system—is not more difficult to learn than French or Spanish, certainly much easier than German.

If we commence with a regular A B C book, a First Reader, Fairy Tales, then read works of the best modern writers—like Bikéla, we shall soon get the aim to acquire understanding and highest pleasure in reading the old Greek classical authors, much better, and without having to undergo the well-known tortures of the present school instruction. If the acquiring of the Greek language is thus made easier, and the classical Greek literature more and more within our reach, Kant's saying will become more obvious: "Even during the dark ages great men have existed. During those periods, however, only those could attain greatness who by nature had been stamped for it. Now, since instruction has been perfected, men are made great by training."

If the Greek language becomes the property of all scholars of all civilized nations in such manner that it may serve as the medium of intercourse, there is no telling how great the practical advantage will be along with the ideal gain. The introduction of the living Greek language into our schools would be of not less significance than the work of the humanists at the end of the Middle Ages.

As the humanists in their times fought against the obstinate and clumsy form in which scholastic science was taught, as they fought against the prevailing professional quarrelling, and the cunning and subtilizing in words,

just as much is it timely now to oppose the unreasonable grammatical instruction in Greek in our schools. These men were inspired for the grand inheritance left by the ancient classical nations; they recognized in this inheritance one of the most excellent means of improvement of the mind, and an inexhaustible soil of noble sentiment.

The single individual can accomplish very little to have justice done to a language which our profession uses already so much in its terminology in preference to any other, to have this language seriously considered when the question of an international language for scholars of all nations is brought up, a language which gives terms to all new inventions and discoveries, and which cannot be replaced by any other, which is already, to a certain extent, an international language.

DISCUSSION.

THE PRESIDENT.—I have listened to Dr. Rose's paper with great interest, and think it is worthy of the widest circulation among educated men. If its statements are verified and his ideas carried out, I think it would be a great step in advance towards rendering our international medical congresses more interesting and instructive than they are at present.

DR. A. JACOBI.—If I had known the nature of the paper which was to be presented to us this evening, I should not have dared to permit the author to put my name on the list of those who were to discuss it. I admit that the enthusiasm of the author for his subject has given me great pleasure while listening to him. The subject is an uncommon one for this floor, and much of it is philological in character, rather than strictly medical. At the same time, it is of direct interest to us, because it contains the proposition that at the international medical congresses the intercourse should be in a common language, and that that language should be the Greek. Dr. Rose's paper, while it is very welcome here, should more properly have been presented at the next International Medi-

cal Congress. I have attended a number of these congresses, and found that whenever a paper was read in the English, French, or German language, only those members remained in the room who understood that particular language; the others would emigrate in large numbers, and thus rapidly thin out the audience. Four years ago, at the congress in Berlin, I was requested to report on the treatment of diphtheria in this country; I began my report in English, whereupon many of the members left the room. At the next congress, in Rome, this difficulty will be still greater. The Italian committee has taken the responsibility of adding a fourth official language to the three already established, and the papers at the coming congress may be read in English, French, German, or Italian. If the congress succeeding this one should be held in St. Petersburg or Madrid, I have not the slightest doubt that the Russians or the Spaniards will add a fifth official language to the present list. The result of this will be that, after a while, there will be no International Congress at all. For this reason, a universal language to be employed at these congresses would be a great blessing. The majority of medical men do not understand and speak more than one language, or perhaps two. It is not a very difficult thing for a child to learn two or three languages at the same time. All of us know families where the children of four or five years of age speak two or three languages. A number of years ago I was acquainted with some Greek families, and in those families every one of the children spoke Greek, English, and French. In one of them, German was spoken also. The English language was learned at school, while the others were acquired at home from the nurse, parents, etc. So far as my own experience goes, I remember that I learned Greek with a great deal of difficulty when I was at school, and that I forgot it with the greatest facility. I believe that many others who studied Greek could give an experience similar to mine. For what purpose should we study Greek and the other so-called "dead languages" to-day? Simply for the purpose of gaining a knowledge of the literature of that language, or for the discipline of mind that it entails? So far as the literature is concerned, I do not know whether we cannot gain just as much from the translation of the writings of Thucydides and Homer

and others as from the original. And so it is also as regards the translation of the writings in modern languages, French, German, etc. The thorough discipline of the mind that is gained by studying Greek is certainly of great benefit to the student.

Let us now take up the practical element of this subject. How is it possible for medical men the world over to communicate with each other more readily than they are now doing? Nor should this subject be confined to medical men. The means of communication between this country and Europe are now so perfect, and the journey can be made in so short a time, that this question is of great practical importance to every one. I believe that much can be gained by better methods of schooling, particularly in the education of small children. I have nothing to propose regarding the matter. I only know that there is such a want of easy communication, and I trust that Dr. Rose's enthusiasm on the subject will be imparted to others, and that the desired end may gradually be brought about by a modification in our present methods of instruction. The author of the paper certainly does not expect medical men to sit down and learn Greek now, either from books or by making the acquaintance of persons who speak that language, as he did. If we want to learn a language, we should begin at the proper time, before we take up special studies. I shall be very glad, however, to learn that Dr. Rose has gained a large number of disciples, and I trust that one of the results of his paper will be that the International Medical Congress of the future will be really international. From what I have seen of them, they are gradually becoming more and more sectional.

DR. CHARLES A. LEALE.—When the accomplished scholar whose subject is before us this evening, called upon me and courteously requested me to speak upon his theme, I answered that probably when I told him instead of the Greek I believed the English would become the language of the future scholar, then he might not desire me to speak; but he still urged me to discuss the subject this evening, even if I differed from him, which I must confess I do, after giving the subject more than twenty years' consideration.

My early professional life began in Washington, the place of meeting of some of the most learned scholars sent

by the different nations of the world as the representatives of their governments and scientific institutions to our country.

I was also brought into contact with many of our own representatives returning from foreign lands, and the officers of our navy, whose duties had called them to all parts of the maritime commercial world.

Before and since that time, travels in foreign lands have brought me in direct contact with people speaking a great variety of languages and dialects.

In early life, I often thought that it would yet be possible for all mankind to believe in a "universal Father;" that we could eventually have a "universal nation;" and as most American boys, I believed that it would be caused by all other nations and peoples eventually seeking and gaining admission into our Union.

I was also taught that there would be no more war; thus peace also would become universal.

I can recall the time when I beheld men, women, and children clothed in spotless white robes, after having sold all they possessed and distributed the proceeds among the poor, stationed on the tops of cold marble tombs in the cemetery, waiting for the coming Messiah promised that night, by the prophet Miller, to come and sit in judgment at the end of the world.

Years have rolled by and these prophecies have not yet been fulfilled; wars and rumors of wars are still here, and we have not yet beaten our swords into plowshares and our spears into pruning-hooks, nor has even the vaunted means of arbitration of differences been followed by the mightiest nations on the earth.

The observing scholar in his travels through different countries, hearing the great variety of languages spoken by the inhabitants, soon concludes that there are peculiarities of soil, climate, and surroundings, as well as varied anthropological developments of the inhabitants, which maintain differences even in languages, while these people remain isolated from the common brotherhood of man.

He is also convinced, that even if by a miracle to-day all human beings upon the earth were made to understand and speak an universal language, and had all traces of any other entirely removed from their literatures and memories, yet in a very few years, probably less than one hun-

dred, changes would occur, demonstrating diverging streams from the main of the original, so great as almost to prevent recognition by the ignorant, who use a language merely as a means of oral communication.

We therefore reach the conclusion that a universal language without continual education would remain an impossibility, and thus we briefly drop the teeming masses of the ignorant, and ascend by one step to consider the fact of the impossibility of establishing the Greek as the language of the scholar, also of the historical fact that the English language not only is rapidly becoming the polite language of Europe but of far-distant Asia, Africa, and America, and that the cultured physician above all others must aim at the highest goal of intellectual attainment. Even after the fall of Constantinople there were many learned Greeks who went to different cities of Southern Europe and became prominent for spreading a knowledge of Greek.

Jebb and Donaldson inform us that "almost all of these men were rather employed about literature than engaged in producing it." They taught Greek; several of them wrote Greek grammars; they transcribed and edited Greek classical writers; and they collected manuscripts.

Bessarion laid the foundation of the library of St. Mark in Venice.

The collection of Constantine Lascaris formed the nucleus of the Escorial library; John Lascaris and his pupil, Budæus, gathered the first stores for the National library in Paris; and Pope Nicholas V. employed the services of Bessarion, Gazis, and C. Lascaris in establishing the Vatican library. But almost none of these men accomplished much in literature, strictly so-called. The question which most deeply interested them was the philosophy of Plato and Aristotle.[1]

Jebb and Donaldson also state that from the earliest times the Greeks were in the habit of putting into verse any remarkable occurrence which stirred their feelings; and within the past month in my office a native Greek scholar confirmed the statement of Crusius, that the Greeks, especially on the islands, contended with each other in repeating or extemporizing verses. This Greek

[1] Ency. Brit. Vol. X, page 149.

gentleman also told me that he had often heard Greek boys and girls under twelve years of age, at his father's home, extemporizing in classical Greek descriptions of surrounding objects and of their joyous sports.

The poetry and culture of the Greeks none can deny; the grandeur of their magnificent architectural monuments we all acknowledge; as we also must those of the Egyptians, whose productions in the time of Moses, and the revelations of their recently interpreted papyri, take us back thousands of years in history.

The rise, decadence, and fall of the Roman Empire, yet too recent to be obliterated from history, teach the philosopher lessons not to be ignored. A visit to the deserted and lascivious temples of Venus, to the voluptuously decorated residences, to the well-paved and clean streets of Pompeii and Herculaneum, reveal to us some of the causes always leading to the death of nations. A visit to the gorgeous structures built for the tyrant Tiberius on the island of Capri, where his Greek slaves were from among the most cultured scholars of Athens, recalls humiliating lessons.

The Greek language was spoken in ancient Greece in its several dialects, as the Attic, the Ionic, the Doric, the Æolic. It is the parent of modern Greek. It has furnished important contributions to the English.[1]

The Romaic language, or its equivalent, the modern Greek, is the vernacular language of about 118,000 descendants of the ancient Greeks dispersed through the Turkish Empire, or residing in the kingdom of Greece. The term Romaic arose from the name Romaioi or Romans, applied to the Greeks during the period of their subjection to the Roman Empire of Constantinople.[2]

In my opinion, the day has passed for Greek to become the universal language for our profession; and I will now endeavor to prove why, instead of Greek, as proposed this evening, English not only should, but has already, become the classic language to be used, not exclusively by the medical profession, but as a storehouse to record all that is learned and useful in the wonderfully developing arts and sciences of the progressive inhabitants of the earth.

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[1] Fowler's Eng. Grammar, page 65.
[2] Ency. Brit. Vol. XI, page 152.

As a proof of the impracticability of introducing Greek as the language of the scholar, permit me to state that even learned Greeks, who have distinguished themselves in medicine, very frequently write their scientific works in French.

"The number of languages spoken on the earth is estimated at eight hundred and sixty, embracing about five thousand dialects.[1] The language spoken by the greatest number of inhabitants is the Chinese; the most widely spread language is the English."

The people of China have no alphabet; every word is a root, and every root is a word. They have not a uniformity of spoken language throughout the Empire; the peculiar dialect of the illiterate class is unintelligible in remote provinces, but their written characters are everywhere the same. The Chinese prefer their mode of speaking to the mind through the eye, by means of visible signs, as superior to spoken words addressed to the ear. Indeed, so far do they carry their attachment to this mode of communication, that it is not uncommon to see men conversing rapidly together by tracing characters in the air.[2]

The most studious scholars in the world are among the Chinese; their systems of competitive examinations illustrate the highest aim of civil reform; the ambition of the poorest boy is to pass his successive examinations, at last become a mandarin, and attain wealth; the great fear of the mandarin or very rich man is, that for some cause he may lose caste and all his property may be confiscated by the government. The Chinese can teach us much that could make us more prosperous and happy, and they repugnantly ignore many European advancements. The intrusion of the Chinese is the dread of the most powerful nations; they are a wonderful people, and have a solitary medium of communication between 400,000,000 of our fellow-men.

The English language is one of the most difficult to learn; from its power and grandeur we accord it a high position. We acknowledge that the original by itself is weak in many ways; but the Anglo-Saxon is now numbered with the Sanscrit, Greek, and Latin among the dead

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[1] Phys. Geog. Home Library, page 401.

[2] Fowler's Eng. Grammar, page 56.

languages. "They are in a petrified state, and they exhibit the modes of thought of the people who spoke them, and their relations to other races, as fossil remains show the forms and relations of animal life."[1]

It is true that the English has retained much that is good from the Anglo-Saxon and Norman; take, for instance, the old Saxon word "self," and to that root, by prefixes and suffixes, we can easily make several hundred useful English words.

There are many things that the scholar can do, but he must not expect to accomplish the unattainable, as finite man can not overcome well-established universal laws. The following facts about the earth are so forcible that I desire to record them in favor of my strong conviction that the English language is the dominant one to-day.

POPULATION OF THE EARTH ACCORDING TO RACE.

(Estimated by John Bartholomew, F. R. G. S., Edinburgh.)

RACE.	Location.	Number.	RACE.	Location.	Number.
Indo-Germanic or Aryan.	Europe, Persia, etc.	545,500,000	Hottentot and Bushmen.	So. Africa.	150,000
Mongolian or Turanian.	Greater part of Asia.	630,000,000	Malay and Polynesian.	Australasia and Polynesia.	35,000,000
Semitic or Hamitic.	North Africa, Arabia.	65,000,000	American Indian.	No. and So. America.	15,000,000
Negro and Bantu.	Central Africa.	150,000,000	Total.....		1,440,650,000

POPULATION OF THE EARTH BY CONTINENTS.

(From Proceedings of the Royal Geographical Society for January, 1891.)

CONTINENTAL DIVISIONS.	Area in Sq. Miles.	INHABITANTS.		CONTINENTAL DIVISIONS.	Area in Sq. Miles.	INHABITANTS.	
		Number.	Per Sq. M.			Number.	Per Sq. M.
Africa.	11,514,000	127,000,000	11.0	Europe.	3,555,000	380,200,000	106.9
America, N.	6,446,000	89,250,000	13.8	Polar Regions.	4,888,800	300,000	0.7
America, S.	6,837,000	36,420,000	5.3				
Asia.	14,710,000	850,000,000	57.7				
Australasia.	3,288,000	4,730,000	1.4	Total.	51,238,800	1,487,900,000	29.0

[1] Fowler's Eng. Grammar, page 42.

The preceding estimate was made by Ernest George Ravenstein, F. R. G. S., the geographer and statistician, and is for 1890.

The average duration of human life is about 33 years. One quarter of the people on the earth die before age 6, one half before age 16, and only about one person of each 100 born lives to age 65. The deaths are calculated at 67 per minute, 97,790 per day, and 35,639,835 per year; the births at 70 per minute, 100,800 per day, and 36,792,000 per year.

EUROPEAN LANGUAGES SPOKEN.

LANGUAGES.	NUMBER OF PERSONS SPOKEN BY		PROPORTION OF THE WHOLE.	
	1801.	1890.	1801.	1890.
English	20,520,000	111,100,000	12.7	27.7
French	31,450,000	51,200,000	19.4	12.7
German	30,320,000	75,200,000	18.7	18.7
Italian	15,070,000	33,400,000	9.3	8.3
Spanish	26,190,000	42,800,000	16.2	10.7
Portuguese	7,480,000	13,000,000	4.7	3.2
Russian	30,770,000	75,000,000	19.0	18.7
Total	161,800,000	401,700,000	100.0	100.0

These estimates by Mulhall (1891) exhibit the superior growth of the English language in the last ninety years. Another authority (see "English-Speaking Religious Communities") estimates the number using the English language in 1893 at over 117,000,000. English is fast becoming the polite tongue of Europe.

For the above statistics, I am indebted to the "World Almanac," 1894.

These statistics may surprise many, when we see by them that proportionately with the languages of Europe, we find a smaller per cent. in 1890, compared with 1801, of French, Italian, Spanish, Portuguese, and even Russian; also that only the same proportion spoke the German language after a lapse of eighty-nine years, while the increased proportion of those speaking the English language more than doubled, having increased from 12.7 per cent. to 27.7 per cent.

In literature and teaching, the German language has been the strongest opponent to the establishment of the English language as a medium of communication of the advanced thought in the medical profession. At the end of the Franco-German war great efforts were made, even

here in America, among the descendants of Germans to place it nearer the first, but the English language has steadily maintained its unapproachable supremacy. We are enabled to glean opinions from modern scholars, whose lives have been spent in endeavoring to solve problems which always vex the human mind.

In modern fiction, we enter with fury places described by a canon of the Church of England in his "Darkness and Dawn," where the Greek slave reads to the Roman Empress Octavia from the classics, while the empress is spinning wool at her distaff. We dream in the "Light of Asia" with the son of the master of old Rugby School of a retrograde civilization to æsthetic self-consciousness; and follow an American army general to distant scenes in the life of Ben Hur. Although these are pleasing recreations and delightful mental enjoyments, yet, when I passed the old towering aqueducts near Rome, walked along the ancient Appian Way, descended into the gloomy catacombs and ascended to the dome of St. Peter's to behold the glories of Rome and the charms of the surrounding scenery; then went to Naples and later to the islands of the Mediterranean, and saw present degradation, filth, and ignorance, and on those classic shores beheld effete efforts to retain the past, I could not refrain from continuing my desire to live under that banner and motto of New York state, "Excelsior."

Several years ago, while the New York Academy occupied its former home, a foreign gentleman of eminence in his own country made a visit to the United States, and during his stay in New York city accepted an invitation to address the medical profession on the special subject of his life's work and publications. Instead of using the vernacular language of the United States while addressing his audience, he chose to illustrate his strong points and thus deeply impress his hearers by resorting to the Greek language. Sentence after sentence flowed like water from his lips.

At that time I was secretary of one of the sections of this Academy, and as the young of our profession were taught that they were at all times at the disposal of their elders, it was my duty to take notes, which was easily done until torrents of classical quotations defied even a mental transposition to the understanding of the listeners.

My hand was paralyzed, my eyes intent on the profound speaker; all present in our over-crowded building, No. 12 West Thirty-first street, appeared hushed in devotion to the wisdom of the Olympian gods. A pin falling to the floor could have been heard. Again our guest, for a few brief sentences, returned to our language to discuss the technical part of one of his special operations; then in pathos ended his oration with what he told us was a classical Greek quotation. He retired to the rostrum, wiped his perspiring brow, smiled in recognition of the applause accorded to him as he took the old arm chair of the Academy.

All was hushed until I noticed a little restlessness in one of our fellows, who, with closed eyes, and deathly pale face, dreamingly occupying a front seat, had apparently just awakened from a long slumber.

He was not our Joseph Jefferson depicting Washington Irving's Rip Van Winkle, but appeared to be a representative of Olympian Zeus as 2,670 years ago returning from the athletic games and races, only celebrated in his honor every four years, he returned to his abode among the gods on the summit of a mountain in Thessaly. Our speaker was one of the future presidents of the Academy, and for more than twenty minutes he repeated accurately from memory paragraph after paragraph of the classical Greek, so erroneously attempted by the previous speaker, not only correcting his bad construction of that beautiful language, but also showing where he had transposed the names of the authorities he had mentioned.

A stranger seated at my right inquired the name of the pale speaker. I answered that he was Dr. Peaslee, one of a large number of representative American scholars, who rarely in speech used a word I could not record. "But in what part of Greece was he born?" I replied that he was born in a small American village of less than one thousand inhabitants, 43 miles north of Boston.

That forensic contest in the New York Academy of Medicine with a former tutor of Greek at Dartmouth College was the last heroic effort to make Greek the means of communication between physicians of different nationalities in this city.

I would not for a moment have this Academy think that I would oppose a study of Greek, as I know without it a

knowledge of the English language would be imperfect, and I have endeavored to instil into the minds of my sons and daughters the beauties of the writings of Homer, who by general consent of antiquity was the first and greatest of poets.

A large number of the best informed physicians find considerable embarrassment in conveying their thoughts to listeners, when standing before a learned audience, even while speaking in the language of their ordinary conversation. How much greater then would be the difficulty if we attempted to use the Greek language. I am afraid that medical science thereby would not be advanced, nor the busy workers in practical life be much edified. We would have more silent, worthy members, and a very few would have the spirit move them to open their storehouses of useful knowledge.

Another and final argument in favor of the American medical profession adhering to the use, improvement, and permanent extension of the English language, is the fact that the largest amount of the current literature of the progressive world is recorded in this language, which has become the most powerful of all.

The works of English and American medical authors written in the English language, which may be found all over the world, attest that we use the best means of communication, and I believe that most of those present this evening would acknowledge, if these works had been written in Greek, if not all, many would never have passed their first editions.

DR. WILLIAM H. THOMSON.—I have only a few remarks to make on this subject. One question that is raised by the author's paper is, whether an international language is practicable, whatever that language may be. I must confess that I do not see any possibility of such a thing, however desirable it may be, unless a revolution occurs in our system of education, and unless all the educators throughout the world coöperate for that purpose. We must remember that the laws of the evolution of language make it absolutely imperative that the language which is to be employed by men for such purposes as those suggested by Dr. Rose—the reading and discussion of scientific papers, etc.—must be learned in such a manner that

we not only can use it, but think in it. The knowledge of a language, in its evolution, is to be divided physiologically into the afferent and the efferent: that which comes through the eye and ear is the afferent, and that which is uttered is the efferent. The language that we learn in early childhood is mainly through the ear; it is only later in life that the eye is brought into use for the purposes of writing and reading. How are we taught Latin and Greek in our colleges? Almost exclusively through the eye; and it is for that reason that the progress made there in the study of the languages is so slow. I can speak very feelingly on this subject. At the age of eleven I began to study Latin, and the following year Greek, and for many years I worked diligently at them both. Later in life I went to Syria, and began to study Arabic in the same way that I had studied Greek and Latin at college, that is, exclusively through the eye. After a few months I made up my mind that I could never learn to speak the language in that way, so I put aside my grammar and all my books, and went about among the Arabs in the bazaars, and listened to them, and learned the language in that way. Now, after twenty-five years, I can speak and think in Arabic with perfect facility, while it is utterly impossible for me to think in either Latin or Greek, although I took the trouble, when I studied those languages, to translate into English all the classical authors I had gone over. I always feel sorry for the young man who is taking a course in French or German at Yale or Harvard. I know what that means, and I know how utterly impossible it is for him to get a useful knowledge of the language by the methods in which it is taught at those institutions.

In order to have a universal language, we must have a universal method of teaching it by means of conversation. As Dr. Jacobi said, our knowledge of languages should be acquired early in life. Suppose all these difficulties were overcome, and it would be within our power to acquire a new language. The question would then arise, Which would be the best international language? and here I must take issue with my friend, Dr. Leale, and fully agree with Dr. Rose. Greek is a spoken language; it is a living language; and if we were to select an ideal language for international communication, I should, for my part, unhesitatingly select the Greek. Its unparalleled and

marvelous structure for the accurate expression of ideas, notably those relating to science, preëminently fits it to serve the purposes of a universal language. It is claimed that there are 25,000 works on grammar in the Arabian language. They have carried the study of grammar and the philosophy of grammar to an amazing degree, and in so doing some of them have shown a surprising insight into the structure of language. One author states that there are three parts of speech: First, nouns, the names of things; second, verbs, the names of events; and third, partitives, which express the relation of things to events. Partitives are the highest parts of speech, because only the mind of man can grasp them. It is exactly on that principle that the matchless structure of the Greek language rests. The English language is deficient in its power of incorporating partitives. In Latin, a single noun may be variously changed to express different ideas by combining it with a prefix; for example, ascription, conscription, description, pre- and proscription, inscription, subscription, superscription, etc. In this respect the Greek is even far superior to the Latin. In our own English, instead of progressing in this way and being able to incorporate one word into another, so as to broaden the idea, we are actually losing many of the expressive words our forefathers used; for example, such words as hereof, whereof, therein, etc., are nowadays seldom employed, excepting in legal documents.

As a scientific method of communication, if it is practicable, I should certainly recommend the Greek language. Although the probability that Greek or any other language will become universal is slight, still it is not impossible. French, at one time, was considered the language of polite society all over the world, and languages, like everything else, are affected by social and political changes. While the proposition made by Dr. Rose will probably not be fulfilled in the near future, it is certainly not altogether chimerical.

DR. S. S. BURT.—I have listened with much pleasure to Dr. Rose's interesting paper, and I believe, with the rest of the speakers, that a universal language is desirable, especially for scientific purposes. It seems to me, however, that a discussion as to the methods of learning a lan-

guage would be more appropriate after the main questions are settled, to wit, Is a universal language possible, and, if so, can the Greek become this language? There is no difference of opinion about the beauty and perfection of the Greek language. I think we all subscribe to the praises which it has received this evening, but these features do not in my opinion determine that a language shall prevail. The dominant language of the world is that which is associated with the people who for the time being are dominant. In this manner the Greek language was very generally spoken when Greece was in the ascendency; and when the Romans held their sway, the same is true of the Latin language. In modern times, French has been a widely accepted means of communication. But now, with the extraordinary spread of English-speaking people over the face of the globe, their tongue is fast approaching as nearly as possible to universality. I think the statistics quoted by Dr. Leale bear out my assertion. That any language, however, will become absolutely universal in this world of conflicting interests, I am inclined to doubt; nor is it possible, in my opinion, to invent a language that will ever gain general acceptance. My reasons for believing in the greater stability of the English as compared with the Greek and Latin languages is based upon Fiske's explanation of the endurance of nations. The Oriental and Roman civilizations failed fully to comprehend government by representation, and herein lay one source of their weakness and final declination.

THE PRESIDENT.—I agree with the reader of the paper, and with the previous speakers, that it would be a very valuable thing if we had at our command a universal language for the dissemination of scientific knowledge; and I think it is perfectly in order, even in a medical society, to discuss the imperfections of the present system of teaching languages, one of which might possibly become the language for universal scientific intercourse. The difficulties at present experienced at the International Medical Congress, as portrayed by Dr. Jacobi, were not overrated by him. Of all the dismal failures that the human race suffers, the International Medical Congress is one of the worst, for the reason that there is no single language of intercourse. No matter how numerous the English-speak-

ing population may be, I do not think that that language will become the universal one so long as France and Germany and other countries exist as powerful nations.

In the face of such statements made by Dr. Thomson, with which we are all heartily in sympathy, it is simply astonishing that the schools and colleges of civilized nations still perpetuate their utterly useless methods of teaching a language. At Yale and Harvard they still continue the farce which prevented so many of us from knowing anything of Latin or Greek, or even of French or German. If I speak feelingly on this subject, it is because I spent a number of years under the most abominable system of teaching ever devised, and during that time, although I worked hard, I learned literally nothing of Greek or Latin that after the lapse of years allows me to read those tongues with fluency, much less speak either of them. It is only by a revolution in the modern methods of teaching that we can ever get a universal language. The hidebound notions of college professors are simply beyond any ordinary assault.

As regards the choice of a language for universal intercourse, I agree with Dr. Rose that that language should not be the English, or the French, or the German, but perhaps the Greek.

DR. ROSE (closing the discussion).—I congratulate myself that my paper has brought out such a full discussion on this subject, and that most of the speakers agree with me regarding the faulty system of instruction in languages now prevalent in our schools and colleges. Dr. Burt stated that the universal language is the language of the dominant people of the earth. I beg to differ with him. The Latin language was the language of the schools all over the civilized world when there existed no Roman Empire.

Stated Meeting, April 5, 1894.

SOME RECENT MEASURES IN THE TREATMENT OF EPILEPSY, WITH SPECIAL REFERENCE TO THE USE OF OPIUM.

A Year's Experience with Flechsig's Plan.

By JOSEPH COLLINS, M. D.

The treatment of epilepsy to-day is about as unsatisfactory as it was ten years ago. That is, a decade that has witnessed a considerable addition to our knowledge of the causation and pathogenesis of this disease has seen no striking addition made to its therapy.

It is not presumptuous to say that, notwithstanding the persistency with which other measures have been advo cated and the extravagant results claimed for them, to-day bromide of potash would not be exchanged for all the other means combined. I do not mean to decry the value of other measures, nor in any way to deprecate their claims in the treatment of this disease, but merely to state what I believe to be a fact.

That there is no dearth of activity in searching for a more curative agent than the bromides in this disease, one can satisfy himself by glancing at the literature of the subject from year to year. Although many agents are recommended, but few are chosen, after a more or less thorough trial. Among the measures recommended within the past few years those deserving of consideration have been more particularly: 1, borax; 2, simulo; 3, antipyrin and antifebrin; 4, nitroglycerin; 5, anti-rabic virus, under which may be included for purpose of con-

venience the "organic extracts;" 6, the various combinations of the bromides.

Among those recommended, but having very little claim for their consideration, are: 1, hypnotism; 2, hydrastine; 3, salicylates; 4, duboisine, picrotoxin, cannabis Indica, belladonna, etc.; 5, amylene hydrate; 6, osmic acid and sclerotinic acid.

Since Gowers recommended the use of borax in epilepsy twenty-five years ago, it has been used more or less in obstinate cases that resisted the bromide treatment, and in cases where for some reason the bromides could not be administered. It has taken a definite place in the therapeutics of this disease, as is attested by many of the most trustworthy clinicians.[1]

Its efficiency compares in no way with the bromides; but as an alternate and adjuvant of the bromides it is, when given in proper doses (about ʒ ij. *per diem*), a useful and valuable addition to our therapeutic resources in this disease. Its efficaciousness is frequently seen in controlling night attacks, while the continuous administration of the bromides prevents the day attacks. The drawbacks to its use are the frequency with which it causes distressing attacks of eczema, psoriasis, impairment of the digestion and nutrition, and even toxic effects. Enthusiastic reports of its efficacy and curativeness now and then crop up,[2] but they are generally accompanied by such meagreness of detail or absence of a judicial weighing of evidence, that they are to be taken with circumspection.

The place held by simulo in the treatment of epilepsy does not compare favorably with borax. Since its introduction by Dr. W. Hale White in 1888 it has apparently never been extensively used. Although White's report of

[1] Folsom: Boston Med. and Surg. Jour., 1886–cxiv–145; Mairet: Le Progrès Médicale, 1892–xv–97; Stewart: Alienist and Neurol., 1891–xii–100; Ferè and Lamy: Nouvelle Iconographie de la Salpêtrière, November and December, 1889; Russell and Taylor: Lancet, 1890–i–1061.

[2] Dijoud: Lancet, 1891–ii–135.

its use in seven cases was encouraging, Eulenberg, who tried it both in hysteria and epilepsy, was not favorably inclined to it. Given in doses of from a half to two drachms of the tincture three times daily, it will, in some cases, especially of haut mal, give gratifying results, as it has done in one patient at present under the writer's observation. It in no way compares with the bromides.

Antipyrin and antifebrin have been used more extensively in epilepsy than have most new remedies, and in many cases with gratifying results. Lemoine[1] was struck by the value of the first of these in a case which always occurred at the menstrual period, in one associated with migraine, and in another of the type of epilepsia larvata or psychical epilepsy. McCall Anderson and Jack report a case of traumatic epilepsy cured[2] in three months by the administration of antipyrin in doses beginning with five grains three times a day and increased one grain a day. The cure was effected when the patient reached a dose of twenty-five grains. It is unnecessary to comment on this case, except to repeat that a case diagnosed, treated, and recorded as cured, all within six months, should not be taken too seriously.

Salm's[3] experience with antipyrin was not so satisfactory. In fact, he thought the results of its use were quite negative. When given in conjunction with the bromide of ammonium, as has been recommended by Wood, it has given very gratifying results and is attested by Potts,[4] [illegible] and [illegible].

A combination of antipyrin with the other bromide salts is probably as serviceable as the combination with

[illegible]

bromide of ammonium. Amade[1] reported cessation of attacks for four months in moderate epileptics under this treatment. The general consensus of opinion in reference to antipyrin and antifebrin is that the former is preferable, and when given in from five- to ten-grain doses three to four times daily it materially enhances the value of the bromides in the treatment of idiopathic epilepsy, particularly in the less aggravated clinical forms.

Nitroglycerin has been used to a considerable degree in idiopathic epilepsy. It has been specially recommended in cases that are attended with vasomotor aura. One of the most recent writers who calls attention to its use is Osler.[2] It is most serviceable in cases of petit mal and must be given in large doses, two to five drops of a one per cent. solution, and in such doses it is apt to produce intensely disagreeable symptoms. Its value as a therapeutic agent in epilepsy is slight.

Attention was first called to the beneficial effects following anti-rabic inoculation of epileptics by the cessation of attacks in two children who had been inoculated for rabies by Pasteur.[3] Other cases showing the beneficial results of this form of treatment have been reported by Charcot,[4] Giovanni,[5] and Ariostigni.[6]

It is distressing, however, to have to repeat that these cases were reported too early to be of very much service in determining the value of anti-rabic virus as a form of treatment.

It should be remarked here that some of the cases (Charcot's) were shown to gatherings of medical men merely to demonstrate the effect the inoculations had in checking the attacks temporarily. It is sufficient comment on this

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[1] Wien. Med. Presse, Nos. 13–15, 1889.
[2] Journ Nerv. & Ment. Dis., 1888–xv–38.
[3] Wien. Med. Presse, No. 24, 1892.
[4] Lancet, 1892–i–1222.
[5] British Med. Jour., September 4, 1892.
[6] Revisita de ciencias medicas, July 5, 1892.

way of breaking epilepsy to say that the results of its use, limited though it has been, and any theory on which its efficacy might possibly be based, have not appealed to the medical profession. A more recent treatment of epilepsy, and one which has, to my mind, absolutely no *raison d'être*, has been introduced by Babes, of Bucharest, and Gibier, of New York. It consists of the hypodermatic injection of the extract of the cerebrum of the sheep, freshly prepared. Although the two investigators quoted above have made serious claims for its efficacy, it is probably no exaggeration to say that this agent is as nearly useless in the treatment of epilepsy as any substance which a physician essays to employ with a considerable éclat can possibly be.

Not even the remotest rational explanation can be advanced for its use. The substance cerebrin has no physiological action which would suggest it to be an anti-epileptic agent. At the present day no one will gainsay the statement that the "organic" extracts depend for any activity that they may possess on their chemical constitution. And when introduced into the system they act, and can only act, just as analogous chemical substances obtained from other sources do.

No analogy between the use of the thyroid in myxœdema or the use of the thymus in pseudo-hypertrophic paralysis can be advanced to explain the use of cerebrin in epilepsy. The brain is not a secretory organ, as are the thyroid and thymus, and its constituents are easily separable by the chemist. These chemical constituents, or as many of them as are not inert, are responsible for any change that the patient has in his symptoms while being subject to treatment by these extracts.

The report of cases of epilepsy treated by this method by Babes is so glowing and enthusiastic, and the statement of his results is so indefinite, that one need make no comment.

Gibier has published two articles in which he details the results that he has obtained. A careful examination of his report will show that the results barely warrant him in his conclusion, a very modest one, "That the method is beneficial in adding to the favorable effects derivable from other therapeutic agents."

Every one who has much to do with the handling of epileptics knows how susceptible they are to new, and especially novel, methods of treatment and how frequently the attacks are for quite long periods amenable to such procedures. The personal factor in the handling of the cases reported by Gibier will probably account to a considerable degree for the amelioration of symptoms that they present. Further, it may be said that there is scarcely a remedy that has been advocated for epilepsy that has not, in the hands of its advocate, given far better results than has brain injection. I do not wish, however, to criticise it harshly, but merely to express an opinion as to its entire worthlessness and uselessness.

The various combinations of bromine, such as bromide of camphor, of nickel, of lithium, of strontium, of arsenic, ethyl bromate, rubidium-ammonium, bromide, etc., have had their advocates and are in many cases of great service, not only as adjuvants to other plans of treatment but as exchange materials for the bromide of potash and soda when the system does not well tolerate the continued tenancy of the latter.

Ferè has recommended the bromide of strontium, but others, with the exception of Deny,[1] who have utilized this suggestion, have not been able to corroborate his good opinion of the drug. Halstead, however, reports with some favor his use of this salt. The bromide of zinc and the bromide of nickel have both been extensively used. The latter, originally recommended by Dr. Costa, was

[1] Semaine Médicale, August 10, 1892.

found to be quite useless by Bourneville,[1] who gave it a very extensive trial. Bromide of zinc has been given by Hammond, Charcot, Bourneville, and other clinicians, but there is so much uncertainty about this salt, both in regard to its dose and its action, that it would be better not to consider its use at all. For instance, it has been recommended by the writers mentioned above in doses varying from five to thirty grains, while a recent number of the *Pharmaceutical Gazette* stated its dose to be from one-half to one grain. Ethyl bromate has been recommended by Donath[2] in doses from thirty to forty drops three times daily and increased to twice this amount if the stomach will stand it. He recommends its administration with oil of peppermint or in capsules.

Corroborative evidence of Donath's estimate of the value of ethylene bromate is given by Olah,[3] who finds that the attacks are milder and less frequent, and that it does not produce bromism. The objections to its use are its extreme volatility and unstableness.

The less important measures against epilepsy that have been enumerated above may be disposed of very rapidly. If our conception of the pathology of epilepsy is correct, or anything approaching correctness, it is an insult to our intelligence to speak of the curative effects of hypnotism in this disease.

The use of hydrastinine, as recommended by Arkhangelsky,[4] has not been successful in the hands of other physicians.

Whatever virtue the salicylates may have in the treatment of epilepsy should be attributed to their action as intestinal antifermentatives. A fact that is too well known to need emphasis is, that the frequency of the attacks of

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[1] Progrès Médicale, 1889–ix–496.
[2] Therapeutische Monatschrift, June, 1891.
[3] Wien. med. Presse, 1891–xxxii–842.
[4] Wratsch, No. 20, 1892.

epilepsy may be diminished often nearly fifty per cent. by treating the alimentary canal exclusively.

Duboisine, picrotoxin, cannabis Indica, and belladonna should in no sense be considered of sufficient importance to attempt to combat epilepsy by any one of them alone. They are frequently of use as adjuvants to the bromides, particularly in combating some untoward symptom produced by the latter. Amylene hydrate, which was first recommended by Wildermuth,[1] has been found by Dunn[2] to be entirely useless, and even dangerous, in the treatment of epilepsy. This conclusion is substantiated by Umphenbach[3] and Drews,[4] who found that this drug greatly increased the mental confusion in epileptics and caused considerable disturbance of sleep. It should be dropped entirely from the therapeutics of epilepsy.

Osmic acid has likewise been shown to be of absolutely no use in the treatment of epilepsy. Sclerotinic acid has been tried by Bourneville and Bricon in twelve cases of epilepsy, and in some of these the frequency of the attacks was lessened.[5] It was noticed that some of the patients to whom the sclerotinic acid was administered decreased rapidly in weight. This fact alone would militate against its use in the treatment of this disease.

Of the non-medicinal ways of treating epilepsy, the eye treatment has received the most attention. All neurologists are keenly aware of the fact that epilepsy may be, and frequently is, excited reflexly and that not infrequently the source of this pathologic reflex is some error of refraction, or defect in the eye muscles, and that the correction of this faulty condition will be followed by the relief of the trouble which it caused. They are likewise aware of the fact that although attacks of epilepsy may

[1] Neurol. Centralbl., 1889–viii–451.
[2] Journ. Ment. Science, 1891–xxxvii–554.
[3] Therapeutische Monatshefte, 1890–iv–499.
[4] Münch. med. Wochenschr., 1891–xxxviii–61.
[5] Arch. de Neurol., xii., xiv., and xv., xvi., 1888.

have been excited originally by defective conditions of the eye, that not infrequently treatment of the eyes alone will not cure the disease, and this for two reasons in particular: First, the eye defect may have been merely the exciting cause in a patient predisposed to epilepsy. That is to say, such a patient might have the epileptogenous areas of his brain brought into activity by many other factors, for instance, by fright, fatigue, over-eating, over-heating, etc., and the relief of such exciting factors would not cure the disease; no more does the relief of the eye defect. In the second place, if a patient has had a number of epileptic attacks, that is, if the motor cells of the epileptogenous areas have been subject to a repetition of explosions, each succeeding one lessens the chances of recovery, and particularly do they lessen the chances of spontaneous recovery on the removal of the cause which excited the first attack. The cessation of attacks goes hand in hand with the improvement of nutrition of the cortical motorial cells, and all our efforts should be in that direction. Not alone the eye but all sources of peripheral irritation should be carefully and intelligently examined and re-examined. Especially should the highly specialized sense organs be examined, and any departure from the *norm* should be labored with till it is overcome. After it has been corrected, the physician will find that all his therapeutic resources are still necessary to combat the disease, which, unfortunately, still continues to exist. It is then that the plan of treatment seeking the bettered nutrition of the epileptogenous areas should be employed to its full extent.

The eye plan of treatment of epileptics, even when looked at with a most charitably critical eye, does not deserve the laudations of most of its advocates. It can never be more than a factor in the treatment of the disease. If we should decide to treat epilepsy on the eye plan perhaps it would be best to have a symposium of medical men, each one of whom would combat the factor which he

considered the important one. Then, when all are finished, the patient will recover in the hands of the physician who gives him iron and arsenic and the iodides, and other measures that will stimulate hæmatogenesis and tissue repair.

When one considers the unsatisfactoriness of the treatment of epilepsy, and the futility of so many of the measures which are periodically lauded as having great and peculiar virtues, it is not surprising that most physicians are animated by a wholesome spirit of scepticism in approaching the trial or consideration of any newly recommended substance. A considerable number are advocated every year, but appeals for their recognition fall on deafened ears, and the freezing reception extended to them cuts them off at their very inception, and thus saves much useless work on the part of the physician, and much harmful experience on the part of the patient. Occasionally, however, a plan of treatment is suggested that appeals to to us in no uncertain way,—one based on a laudable hypothesis, or one that involves no shriveling of our intellect to understand how it may act, or one that has been tried by an observer who from his labors and reputation deserves to be heard, and who recommends it because he has seen it do good, and offers it to his fellow-workers without explanation. It is to such a plan of treatment that I wish to invite your attention.

About a year ago, Professor Flechsig, of Leipsic, published a short article on a new method of treating epilepsy, which in his hands had given most gratifying results. It consists in administering opium in the shape of the extract or pill in large doses for a period of six weeks. The dose of opium in the beginning is from one-half to one grain, and this is gradually increased until the patient is taking fifteen grains or more per day, in doses of from three to four grains. The maximum dose is reached by the end of the first week. At the end of six weeks the opium

is suddenly stopped, and for it bromide of potassium or sodium in doses of one-half drachm four times daily is substituted. After these large doses of bromide have been kept up for some time, the dose is gradually decreased until the patient is taking less than two scruples per day. The sudden cessation of administering the opium, and the exhibition of the bromide is quite essential.

Being cognizant of the fact that opium in small doses when combined with bromides is frequently of great service in lessening the severity of attacks of epilepsy, the plan of treatment suggested by Flechsig appealed to me, and I determined to give it a thorough trial in a large number of cases selected from private, hospital, and dispensary practice, and to examine the results of such treatment at the end of one year. About fifty patients have been subjected to this plan of treatment. A few cases I shall detail, and content myself with presenting the results observed in the others in tabular form.

The twenty cases considered in the first table I had under almost daily observation, and the conclusions reached regarding this plan of treatment are based largely on the facts therein set forth. The sixteen cases considered in the second table I did not have under such close surveillance. They were very old subjects and my principal desire in subjecting them to the treatment was to determine whether the results would militate against, or corroborate, the results obtained in the first twenty cases. In a general way, it may be said that the results were not so satisfactory.

TABLE I.

No.	Name.	Age.	Sex.	Duration of Epilepsy.	Frequency of Fits under Bromide Treatment.	Character of Attack.	Cessation of Fits after Opium.	Frequency of Fits after Relapse.	Remarks.
1	F. M.	14	Fe.	3 years.	Once a week.	Haut and petit mal.	3 months.	Once in 3 months.	Dispensary patient.
2	H. N.	22	M.	6 "	1 to 2 "	Haut mal.	5 "	1 a month.	Private patient.
3	M. S.	9	Fe.	6 "	1 to 3 "	Haut and petit mal.	5 "	No relapse.	Dispensary patient.
4	M. P.	21	M.	1 year.	2 to 3 "	Haut mal.	8 "	" "	" "
5	H. B.	16	Fe.	6 years.	6 "	Haut and petit mal.	2 weeks.	Every 4th day.	Private patient.
6	A. F.	22	M.	8 "	7 "	Haut mal.	6 "	1 a month.	Hospital patient.
7	J. N.	16	M.	9 "	9 "	Haut and petit mal.	4 "	1 a week.	" "
8	E. S.	23	M.	20 "	6 "	" " "	7 "	2 a month.	" "
9	C. F.	27	M.	9 "	2 "	Haut mal.	8 "	Every 3 weeks.	" "
10	A. S.	26	M.	25 "	2 "	"	1 week.	1 a month.	" "
11	L. M.	21	Fe.	7 "	1 "	"	5 weeks.	2 a month.	" "
12	E. R.	20	Fe.	4 "	3 "	Haut and petit mal.	2 "	Once in 2 weeks.	" "
13	J. R.	27	M.	10 "	1 "	Haut mal.	4 "	" 3 "	Dispensary patient.
14	L. B.	15	Fe.	3 "	3 "	"	7 "	" 2 "	" "
15	B. D.	24	Fe.	1 "	1 "	"	5 months.	" 4 "	Private patient.
16	M. S.	28	Fe.	11 "	2 "	"	5 weeks.	1 a week.	Hospital patient.
17	D. M.	21	Fe.	5 "	4 "	Haut and petit mal.	7 "	3 a month.	" "
18	E. H.	27	M.	7 "	2 "	Haut mal.	6 "	3 "	Private patient.
19	M. Mc.	19	Fe.	5 "	4 "	Haut and petit mal.	3 "	1 a week.	Hospital patient.
20	J. L.	17	M.	6 "	3 "	Haut mal.	1 month.	2 "	Dispensary patient.

TABLE II.

THE FOLLOWING SIXTEEN CASES HAVE BEEN UNDER OBSERVATION FOR FOUR MONTHS ONLY.

No.	Name.	Age.	Sex.	Duration of Epilepsy.	Frequency of Fits under Bromide Treatment.	Character of Attack.	Cessation of Fits after Opium.	Frequency of Fits after Relapse.	Remarks.
1	D. S.	48	M.	13 years.	2 daily.	Haut mal.	1 week.	4 a week.	Hospital patient.
2	J. K.	26	M.	12 "	4 a week.	Haut and petit mal.	No impr'vem't.	.....	" "
3	W. D.	40	M.	20 "	3 "	" " "	" "	.....	" "
4	J. L.	27	M.	8 "	1 "	Haut mal.	2 weeks.	1 in 2 weeks.	" "
5	G. Y.	38	M.	11 "	5 "	Haut and petit mal.	11 days.	2 a week.	" "
6	T. M.	64	M.	26 "	7 "	" " "	6 days.	3 "	" "
7	S. H.	39	M.	14 "	3 "	" " "	2 weeks.	3 "	" "
8	W. M.	50	M.	18 "	10 "	Petit mal.	17 days.	2 "	" "
9	J. O.	22	M.	10 "	2 "	Haut and petit mal.	10 "	1 "	" "
10	J. K.	26	M.	7 "	2 "	" " "	3 weeks.	3 a month.	" "
11	J. W.	61	M.	22 "	3 "	" " "	13 days.	1 a week.	" "
12	B. G.	38	M.	15 "	5 "	Haut mal.	No impr'vem't.	.....	" "
13	W. H.	46	M	6 "	2 "	"	5 days.	2 a week.	" "
14	D. M.	45	M.	11 "	3 "	"	15 "	1 "	" "
15	H. C.	55	M.	20 "	6 "	Haut and petit mal.	9 "	2 "	" "
16	J. D.	51	M.	17 "	3 "	" " "	3 weeks.	2 "	" "

A study of Table I shows that all the cases, with one exception (case 5), were benefited. It further shows that in two cases (cases 3 and 4) the fits have not returned. The time elapsed in these cases is, however, not of sufficient length to refer to them in any way as cured. And I wish to emphasize the point that, although the attacks have ceased, I do not consider them cured, and they are still taking four scruples of potassium bromide a day. In several of the cases the character of the attack after the relapse was frequently of a different nature from that before the attack. That is, the family of the patient or the hospital nurse, as the case might be, would describe them as "mere fainting spells" and make light of them. I have observed, however, as time went on, that these "spells" gradually became more severe as the number of attacks increased after the relapse. Another fact which I would point out is, that in every case, almost without exception, the maximum dose of opium was reached and persevered in with comparatively slight trouble. Some of the patients who were cognizant of what they were taking complained rather bitterly and rebelled, but particular attention to the bowels and a good deal of outdoor exercise served to keep them moderately comfortable. Sometimes it was necessary to administer strong coffee to combat the drowsiness, particularly during the first week, when the opium was being increased rapidly.

While the patients were taking opium there was no marked change in the frequency of the fits. Some of the patients had a lesser number, particularly toward the latter end of the six weeks' period, while others had them with customary frequency.

Notwithstanding the fact that no sinister results accompanied the administration of opium in such large doses, I feel it incumbent on me to say to any one who would apply this plan of treatment, that the most watchful and scrutinizing care is necessary, especially during the

first weeks, until the patient becomes accustomed to the large doses.

The most satisfactory results were obtained in very chronic epileptics, and particularly those who were not responsive to large quantities of bromide. In epilepsy dependent on, or associated with, gross organic lesion of the brain the treatment seemed to give better results than in pure idiopathic epilepsy. By gross organic lesion, I mean epilepsy associated with old cerebral hæmorrhage and softening, and epilepsy associated with defective development.

I can but state my convictions relative to this plan of treatment in the following conclusions:

1. The plan suggested by Flechsig is not a specific in the treatment of epilepsy.

2. In almost every case in which this plan of treatment has been tried there has been a cessation of the fits for a greater or lesser time.

3. A relapse generally occurs in a period varying from a few weeks to a few months.

4. The frequency of fits after the exhibition of opium is, for the first year at least, lessened more than one half.

5. The attacks occurring after the relapse are much less severe in character than those that the patient has been accustomed to have.

6. This plan of treatment is particularly valuable in ancient and intractable cases.

7. In recent cases of idiopathic epilepsy it cannot be recommended.

8. The opium plan of treatment is an important adjuvant to the bromide plan as ordinarily applied.

9. The opium acts symptomatically, and merely prepares the way for, and enhances the activity of, the bromides and other therapeutic measures.

10. This plan of treatment permits the use of any other

substances which are known to have a beneficial action in epilepsy.

I will cite briefly the history of a few unselected cases taken from hospital, dispensary, and private case books, showing the effect of the treatment. I may say that all of these patients were handled with bromides for a longer or shorter time previous to being put on the Flechsig plan.

CASE I.—Dispensary.—F. M., female, colored, aged fourteen. Family history, tubercular; personal history, good. Never had convulsions when a child; was always bright, vivacious, and smart; talked early and walked early. First attack when eleven years of age; cause, unknown; character of attack, haut mal. Frequency of attacks, about three a month and getting more frequent. Character of aura, headache for some hours. Physical condition, good; mental condition, backward; she has a great many evidences of mental deterioration. Head symmetrical; no other somatic evidences of degeneracy.

Put on opium treatment September 20th. Reached thirteen grains a day at end of first week. At this time took thirteen pills at one time and suffered from toxic effect, but recovered without difficulty. Put on kalium bromide, ʒ ss, 4 i. d., November 1st. Patient reported once a week till January, then stopped for two weeks. Returned February 8, and said she had had a fainting spell, while the medicine had been stopped. Bromide in 20 gr. doses 4 i. d. continued. No attacks since that time. Result: One attack of petit mal in six months. Mental condition, about the same.

CASE II.—Private.—H. N., male, aged twenty-two, salesman. Maternal side of family, neurotic heritage. Paternal great-uncle had epilepsy. Attacks began when he was sixteen years of age. Cause, not attributable. Had one spasm when a child, caused by eating unripe fruit. Was bright mentally and strong physically, as a boy. Character of attack, haut mal. Time of occurrence, always at night. Frequency, in the beginning they occurred about once in six months; now they come once or twice a week. Mentally, he is deteriorating, forgetful, depressed; cannot concentrate his mind. Physically, not robust.

Put on opium treatment, April 30th. Maximum dose, fifteen grains, reached in eight days, and continued till June 16th. No change in attacks in point of form or frequency while taking opium.

June 15th.—Substituted bromide of potassium, ʒ ij. per diem, for opium. Reports once a week.

November 28th.—Three attacks in succession the same night. Had not been taking medicine for five days.

December 18th.—Nocturnal attack.

March 27th, 1894.—Reports one attack since December.

Result: Four attacks in nine months. Physical and mental improvement.

CASE III.—M. S., female, nine years of age. One sister died of hydrocephalus. She did not walk till two years of age, and began to talk when six. Attacks first began at the age of three years. They were then of the petit mal character, and gradually increased in frequency until her eighth year, when they were occurring once or twice a week. From her eighth year, she has had both petit mal and grand mal. Mentally, the patient is quite defective. Evidence of somatic degeneracy well marked.

Patient put on opium, gr. j., t. i. d. for three weeks; then on double the quantity for the next three weeks; then on triple the quantity for the next four weeks. Middle of November, put on potassium bromide, 30 grains four times a day.

It is necessary to mention that this little patient was the only one of the entire number that developed symptoms of the opium habit. On substituting the bromides for the opium, such symptoms of the opium habit as diarrhœa, anorexia, restlessness, irritability, and sleeplessness showed themselves, but were easily combated with hydrotherapy and small doses of chloral. She has had no attacks for upward of five months. Mentally, there is some gratifying improvement. She helps her mother about the house; can be trusted to go errands and the like. During the administration of the bromide she has had restoratives, such as iron and cod-liver oil, in such quantities as she could dispose of.

CASE IV.—M. P., male, twenty-one years of age; heavy drinker. One brother had attacks for upward of twelve years and recovered at the end of that time. One

year ago, after a heavy drinking bout, he had an attack of pneumonia, and while convalescing from this he had his first epileptic attack. After this, they occurred two to three times a week, and both in the day and in the night. Has an aura starting from the stomach, accompanied by cardiac palpitation. Mental condition, good.

Put on opium August 22d, and continued for six weeks; then bromide in 30-grain doses four times a day. Result: No attacks since cessation of opium.

CASE V.—H. B., female, sixteen years of age. Mother died of tuberculosis. Had convulsions when three and a half years of age; then was quite free from attacks till ten years of age. Talked and walked early, and learned easily until her tenth year, when she had severe attacks of haut mal. Three or four months later she had a second attack, and since that time they have increased in frequency. Concomitant with the increase in the number of attacks has been mental deterioration, so that now she is quite demented. Three years ago she had an attack of chorea, and while ailing with this, she had a left side hemiplegia which disappeared in about a fortnight. Her mind has been gradually getting weaker, and for the past six months she has had visual and auditory hallucinations. Always before an attack she complains that her eyes hurt her. Evidence of somatic degeneracy, such as stunted stature, torsus palatinus, over-exposure of gums, defective lobulation of ears, crowding of teeth, broad, flat, nasal arch very apparent. For the past six months she has had at least six attacks a week; for two or three hours after an attack she is entirely *non compos mentis*.

Put on opium January 10th, 1894, and for the next six weeks, while on opium, the attacks were not so frequent as formerly.

February 28th.—Put on kalium bromide. No attacks for two weeks; since then, an attack about every fourth day.

CASE VI.—A. F., male, aged twenty-two, of neuropathic heritage. First attack of epilepsy when fourteen years of age. Attributing cause, a severe fright. The fits occurred about every three weeks for the next four years. Since that time the attacks have been getting more frequent, although he has been under continual treatment. In the month previous to the beginning of the opium treat-

ment he had twenty-seven attacks—seventeen day attacks and ten night attacks—all of the haut mal type. Patient put on opium May 25th, 1893.

The following is a record of his attacks while taking the opium: May 25th, six attacks; June 10th, two attacks; June 11th, one attack, followed by a maniacal outbreak. Rectal injection of 30 grains of chloral hydrate and 60 grains of bromide of potassium, hypodermatic injection of 1-100 grain hyoscyamine hydrobromatis, and ice-cap to the head.

June 12th.—Patient rational.

July 18th.—Opium stopped, and given instead bromide of potassium, 30 grains four times a day.

August 27th.—Two "fainting" spells.

October 24th.—Petit mal.

October 25th.—Petit mal.

November 3d.—Petit mal.

December 2d.—Two attacks of petit mal.

January.—No attacks. Patient left the hospital.

CASE VII.—J. N., male, sixteen years of age, no family history. Has had fits since seventh year. Cause, severe fright. The form that the attacks take is variable; occasionally they are of procursive type, sometimes petit mal, and sometimes very severe attacks of haut mal. In the month preceding the opium treatment, he had thirty-eight fits. Mentally, patient is entirely demented. Opium stopped and bromides substituted July 25th.

The following is the record of attacks for the remainder of the year: August, four attacks; September, one attack; October, three attacks, all of petit mal type; November, five attacks; December, three attacks, one of the haut mal type. Patient transferred to the asylum.

CASE VIII.—E. S., male, twenty-three years of age. Negative family history. Has had fits since childhood. Attributable cause, fright. Mentally the patient is morose, stupid, wrathy, and scarcely ever speaks unless angered. In month previous to establishing the opium treatment, he had twenty-four attacks. Opium stopped July 25th, and bromides substituted. The following is the record: September, 1893, two attacks; October, two attacks; January, 1894, two attacks; February, one attack; March, three attacks.

This patient has undergone a remarkable mental change for the better, is now cheerful and bright, and works continually about the ward.

CASE IX.—C. F., male, twenty-seven years of age. Negative family history. Has had fits since he was eighteen years of age. At first they occurred at night, but for some years past he has had both night and day attacks. He has about eight attacks a month, in each one of which more than an hour elapses before he recovers consciousness. Mentally the patient is deranged, and spends most of the time praying. He received the opium in the regular way, and was put on bromides August 2d, 1893. He suffered no attack up to October 1st, when he was transferred to the surgical ward, that he might be operated on for a malignant growth of the cheek. Since then he has had a few light attacks. Mentally, he is greatly improved.

CASE X.—A. S., male, twenty-six years of age. Paternal uncle has epilepsy. This patient had convulsions when a child, after an attack of scarlet fever. When ten years of age, had his first severe fit. He has been under almost continual treatment in this country and Germany since his boyhood, but the attacks have become more frequent and severe. Sometimes he will have three or four attacks in a day and then will not have a repetition for a week. Most of the attacks are in the day. Mentally and physically, the patient is in fairly good condition. Patient put on opium June 10th and stopped August 2d.

The record from that time is: August 6th, two "fainting" spells; November 28th, two attacks of petit mal; February 5th, 1894, one haut mal; February 10th, petit mal; March 7th, petit mal.

In conclusion, it gives me great pleasure to acknowledge the many valuable suggestions received from Dr. C. L. Dana relative to the prosecution of this trial, and to thank him for placing at my disposal the extensive material of his clinic at the New York Post Graduate Medical School.

To Dr. E. D. Fisher, my best thanks are due also for allowing me the privilege of his wards at the almshouse.

The valuable and untiring aid rendered me by my successive house physicians, Drs. Hedges, Grandy, and Worth, at the Hospital for Nervous Diseases, is gratefully acknowledged.

DISCUSSION.

DR. E. D. FISHER.—For a number of months past, I have seen several of the cases of epilepsy which Dr. Collins has had under his charge. He certainly chose the most intractable cases from among those that have been under my observation, most of them having been subject to epilepsy for years. Under the treatment of these patients, as instituted by Dr. Collins, the number of their severe attacks has been very much reduced. The milder attacks were not so much benefited. The character of the attacks of the haut mal type seems to have been somewhat altered. The general intelligence of the patient in some cases was reduced rather than improved under the opium treatment. It struck me that the stupidity so frequently observed in these patients was rather increased. They certainly became much more tractable, and were more easily taken care of.

In regard to the ultimate results to be gained by this method of treatment; whether we can hope that it will effect a permanent cure in epilepsy, I have very grave doubts. It strikes me as a good method to be employed in the very severe class of this almost incurable disease, and I am in favor of it in such cases. I would choose the chronic cases rather than the acute ones for its application. I do not think the opium exerts any deleterious effect on these patients, nor is there much to be feared from the protracted treatment that is necessary, if the drug is carefully administered. At first, the treatment seemed to me to be rather heroic. I would like to ask Dr. Collins if he has formulated any theory as to how the drug acts in these cases.

DR. F. PETERSON.—In our joint service at the New York Hospital for Nervous Diseases, I saw a number of these cases of epilepsy, which were under Dr. Collins's care, and have had a number of private cases under the opium treatment. I would say that Flechsig's method, while not in any sense curative, is a distinct gain in the

therapeutics of epilepsy. In all chronic cases where ordinary treatment fails, it is indicated and will usually be of great benefit.

DR. W. H. THOMSON.—I beg to say that I have always taken special interest in the subject of epilepsy, inasmuch as it was a case of that disease that prompted me to choose medicine as my profession. A teacher of mine was one day taken with an epileptic fit, and I thought it would be a good thing to belong to a profession that could cure such a terrible disease. I have since then paid a great deal of attention to the pathology and treatment of this disease, and I may say that my views regarding its treatment are not much different to-day from what they were a number of years ago. I have no faith whatsoever in the curative properties of any of the so-called nervine remedies. My reason for this is, that I regard epilepsy, in the majority of cases, as a very deep-seated disease, as a constitutional malady rather than a functional one, and that we make a serious mistake when we look upon the symptoms as the disease itself. Many people refer to epilepsy as a convulsive disease, because the most striking symptom is the occurrence of convulsions. I have records of a number of cases in which there were rarely, if ever, any convulsions, and no loss of consciousness, and yet they were true cases of epilepsy. We have a large number of them, also, that have been recognized under the name of petit mal, in which the convulsions are either absent or are a very insignificant symptom. The convulsions, therefore, are merely an occasional accompaniment of the disease. There is something besides convulsions which is the real element in the disease. There is only one constant element in epilepsy, and that is, the suddenness of the attack. This suddenness may simply consist of a sudden loss of speech, as in one case I have seen, which may last for two or three hours and then pass off, accompanied absolutely with no other symptoms. In petit mal, the attacks are of sudden onset, as they also are in the more severe type with convulsions. Whenever we have an obscure disease to deal with, I think it is a good plan to find out what the invariable element of that disease is, and with epilepsy, that element is certainly the sudden onset of the seizure. Everything else about the disease is as protean as can be.

Outside of the suddenness of the attack, no two epileptics will give you the same set of symptoms.

I regard the trouble in epilepsy as essentially a sudden interruption of the normal inhibitory action between the cerebral cells upon each other, and that this is always the result of an abnormal afferent impression. I believe there is no such thing as a spontaneous explosion of one of the cerebral cells, but that such abnormal explosion is always brought about by an abnormal afferent impression. Hence every impression of an unusual character is dangerous. This is seen in the very violent and sometimes fatal convulsions that occur at times on washing out the stomach or pleura in persons who never had epilepsy. I do not believe that any nervine will prove curative in epilepsy. Opium, aconite, belladonna, and similar drugs produce their full effect by one dose, and if you give a thousand doses, they will not do more than the first. Take a spasmodic nervous disease like asthma, which is said to be relieved by one of the mydriatics, such as belladonna or stramonium; does it relieve the asthma? No, it relieves the symptom of asthma, the difficult breathing, and it will not do more if we give it for twenty years. There is nothing progressive in the action of these drugs. The only remedies for asthma are iodine or arsenic. So it is with epilepsy. Whoever is familiar with the subject of epilepsy knows very well that these patients often seem to be benefited for a time when a new method of treatment is undertaken. I have no doubt that Flechsig, when he first commenced to treat these cases with opium, thought that he had hit upon a very good remedy, and I also venture to say that within one or two years he will abandon this method of treatment. The relief obtained is when the patients are first put upon the drug. Gradually, the relapses will increase in frequency, and after one or two years the treatment will fall into disuse. The nervines are not of any great benefit in epilepsy, and are to be used only as adjuvants. In certain cases of epilepsy, opium used in a certain way and for a certain time as an adjuvant may prove serviceable. It is a functional remedy, not a constitutional one. Opium will produce no changes that the pathologist can discover, no matter how long the drug is administered; in that respect it is similar to tobacco, which is also a powerful nervine.

As regards the treatment of epilepsy, I have never yet seen any drug which is equal to the bromides, properly administered. The trouble in many cases is that the bromides are given too much in the routine way. The bromides are just as much a specific for epilepsy as mercury is for syphilis, but they should not be given in a routine way. Unless they are administered intelligently, sooner or later they will fail, just as injudicious mercurial treatment will fail in syphilis. I doubt whether opium will do as much good in epilepsy as antipyrine or chloral as an adjuvant to the bromide treatment, and in cases with symptoms of a focal character, I certainly think it is inferior to conium. In the treatment of these cases, it is very important to discover what part of the afferent nervous system is deranged, and in about one half of the cases the trouble is discovered somewhere in the alimentary canal. I invariably combine cod-liver oil with the bromides, and then, as an adjuvant, use the nervines. Chloral and antipyrine are very serviceable for this purpose, and conium where there are signs of focal disturbance. There is a further objection to the use of opium. I do not see how, in the treatment of such a chronic disease as epilepsy, we can continue the steady use of the opiates without engendering the opium habit. We may be able to control the patient so long as he is under our care, but afterwards, when he passes from under the surveillance of the physician, I should be very much surprised if he did not take the matter in his own hands.

DR. COLLINS.—I have very little to say in closing. In answer to Dr. Fisher's statement concerning the impaired intelligence of the patients who are subjected to this treatment, I desire to say that I have not observed any very deleterious effects following its use. One or two became maniacal, but that, I think, had nothing to do with the opium treatment, as the mania occurred while the patients were on the bromides, and some time after the cessation of the opium. In a number of the patients, particularly those who had previous infantile hemiplegia, the intelligence was materially increased.

I think Dr. Peterson has echoed my sentiments regarding the value of opium in the treatment of epilepsy. In the first part of my paper, I subjected the other plans of

treatment to rather severe strictures, and I do not think that the plan advocated by Flechsig is entirely free from those same objections. I have tried to contrast it with the other remedies, and I believe that next to the bromides it is the most valuable that has been advocated. It does not preclude the administration of the bromides, and it does prove a valuable adjuvant to them.

The cases reported in my paper have been under my observation for one year. More than one half of them were in the hospital, and could not get away. In all of them, with the exception of those in the almshouse, the opium was discontinued six months ago, and nothing in the behavior of those patients would lead me to believe that they would buy opium, even if they were rich as Crœsus, and free as air.

It is true, as Dr. Thomson said, that opium is a functional remedy, causing no anatomical changes which can be discovered. It is also true, that although advanced pathology does not regard epilepsy as a functional disease, we know of no anatomical changes in the brain to which we can attribute it. Even if opium does produce its effects by one dose, those effects can be renewed by a repetition of the dose, and if it assists the bromides in preventing the epileptic attacks, then to that measure is it curative in epilepsy. Physiologically, opium produces a congestion of the brain, while the focal discharge in epilepsy produces cerebral anæmia. It seems reasonable to suppose that by keeping up the circulation of the brain with opium, the epileptogenous discharge can be obviated. Why should we not influence the disease if we influence the blood supply? By influencing the blood supply and its nutritive properties, we cure other organic diseases. The statistics in my paper I give you for what they are worth. They represent moderately honest work. I have subjected them to a very severe criticism before I read them here to-night. I believe, with Dr. Thomson, that opium will not take any great place in the treatment of epilepsy, but I also believe that it is superior to the other remedies I have mentioned. If disturbance of the alimentary tract exists in any of the cases, it certainly should be corrected. Frequently, the administration of proper remedies for this purpose will reduce the number of attacks by one third. If by the use of opium we can still further lessen the

number of the attacks, we are doing much for our patient. We know that every attack that an epileptic has decreases his power of resistance against future attacks. When an epileptic patient has fit after fit, sooner or later he will get into a condition where no medicine will cure him, and it is that class of patients that we find in the almshouse and the Hospital for Nervous Diseases, and from among them most of my cases were selected.

In conclusion, I wish to say that I do not advocate the opium treatment as curative in epilepsy. It does not compare in value with the bromides, but I consider it superior to any of the other adjuvant methods.

THE PRESIDENT.—I am a little surprised to learn that none of the speakers referred to the articles which recently appeared in a New York medical journal, in reference to the treatment of epilepsy by cutting the muscles of the eye, and fitting proper glasses. Evidently, that method of treatment is not received with as much favor by the neurologists as I had supposed.

APPENDICITIS STRICTLY A SURGICAL LESION.

BY JOHN A. WYETH, M. D.

It is not my intention to go into the pathology, symptoms, diagnosis, or operative technique of appendicitis. There is practically nothing to be added to such contributions to the literature of this subject as the paper of Professor William H. Welch in the *Transactions of the Congress of American Physicians and Surgeons*, vol. ii, page 5; "The Importance to the Surgeon of Familiarity with the *Bacillus Coli Communis*," by Professor Roswell Park, in *Annals of Surgery* for September, 1893; a classical paper, "Observations upon Appendicitis," by Dr. George Ryerson Fowler, in the same paper for January, February, March, and April, 1894; "Remarks upon Appendicitis," by Maurice H. Richardson, M. D., in the *American Journal of the Medical Sciences*, January, 1894; "Remarks on Appendicitis," by Professor Christian Fenger, in the *American Journal of Obstetrics*, August, 1893; "Ueber Aetiologie und Pathogenese der Peritonitis durch Perforation," by Barbacci, in *Centralblatt für allgemeine Pathologie*, 1893; and several practical and excellent articles by the late Professor Sands, and by McBurney, Fitz, Weir, Baruch, Keen, Bull, Bryant, Senn, and others.

Suffice it to say that the danger to life which makes appendicitis strictly a surgical lesion is *peritonitis*, and this may occur not only with, but without, perforation. Through any portion of the walls of the appendix, the vitality of which is materially impaired by ulceration or gangrene and yet not perforated, septic micro-organisms or ptomaines from the alimentary canal may pass and thus precipitate septic peritonitis, local or general. That gases

also pass through is evident from the peculiar fæcal or intestinal odor imparted to the contents of the abscess or cavity of the peritonæum, when the most careful search shows no perforation is present.

When perforation exists, the septic process is rapid and, unless encapsulated by the accident of peritonæal adhesion, soon induces a fatal termination.

In all the range of the materia medica there is not an agent which can prevent peritonæal infection from a diseased appendix, nor is there an agent which exerts a curative influence over this disease when once established. In the presence of this most grave malady the physician is powerless save in the allaying of pain, the assurance of quiet, and the conservation of the patient's strength by proper nourishment. If the disease is mild in type, the inflammatory process limited, and resolution occurs, he congratulates himself, and is to be congratulated for his luck rather than judgment. If perforation and general infection or abscess, or obstruction by adhesions occur, he has recourse to surgery at a time when the hand which earlier might have interfered for the patient's safety, is often invoked too late.

In my entire experience with this lesion I have yet to see a death which could not properly be ascribed to delay in timely and skilful surgical interference. With all respect to the diagnostician, I do not believe it is within the scope of human judgment to determine from the symptoms whether or not perforation or gangrene is about to occur, whether peritonitis with or without perforation is being circumscribed by adhesion or spreading to general infection. Every surgeon of experience is familiar with such cases. I do not mean to say that by a careful study of the symptoms these conditions may not be made out in a fair proportion of cases, but I do mean to say that, as at present treated by our profession, the rate of mortality is greater than it should be, and I do know from my own

experience that prompt surgical treatment will give a better ratio of recovery.

In a short paper based on eleven cases treated in the month of September, 1892, and read before the Surgical Section of this Academy and in a subsequent report, I maintained this position, and if you will bear with me in a condensed recital of more recent cases, I will still further strengthen the conclusion that every case of appendicitis from the first dawn of a symptom of the disease should be treated by a surgeon, with the regular medical attendant.

On February 6th, 1894, I was called to Astoria in consultation with Dr. H. O. Fitch and Dr. J. D. Trask. The patient, A. B., aged twenty-one years, was an athletic, robust man, with no previous disease. He had had two or three attacks of "cramps" in the bowel within the last two years, but there was no pain in the right iliac region, no tenderness, or no muscular resistance. The pain in these attacks was felt at the umbilicus, and a small dose of morphine had in each instance relieved him, and on no occasion was he kept from his business.

On Monday, February 5th, he had dined heartily in New York city, reached his home by twelve, midnight, and retired as well as ever in his life. At 2 a. m., he was awakened by a pain in the right iliac fossa and sent for Dr. Fitch, who saw him at 7 a. m. This time the pain was well marked at the appendix. The temperature was 101.5° F.

I saw the patient at 2.30 p. m. He was lying on his back with the legs in full extension. The temperature was not quite 102° F. There was some muscular resistance, which yielded when the muscles were relaxed at will; pain not severe on pressure. He had not vomited and no morphine had been administered. Deep pressure recognized a small resisting body at the location of the appendix. There was not a symptom in the case which might not have excused operative interference with the hope of resolution. Knowing as I did, with such a robust subject, that exploration was not dangerous, and knowing, as I have had sad reasons for remembering, of many cases in which

the patient perished from timidity and delay, I advised operation, and Dr. Fitch and Dr. Trask promptly coincided.

The appendix was found greatly distended with soft semi-solid ingesta, coiled on itself, as shown in this picture, and deeply mottled in its whole extent and evidently becoming rapidly gangrenous. It was not perforated. I tested this by forcing water into it after removal, and, under pressure, it was impermeable. On the contiguous surfaces of the colon and ileum yellow films of peritoneal lymph with a covering of creamy pus was seen. The first ligature applied to the appendix broke through the gangrenous wall, and the final ligature was tied at the line of the cæcum. I do not give the operative technique, for it is too well known to require repetition here. The recovery was uninterrupted.

The appendix vermiformis coiled on itself, and gangrenous thirteen hours after the first symptoms of appendicitis.
***a, a*, the appendix imprisoned in a mass of organized lymph.**

In this case had operation been delayed, rupture of the appendix was inevitable within a few hours, and there was no chance of encapsulation by adhesions. Death would surely have followed.

It so happened that on the following day I was called to see a boy, five years old, hitherto healthy and robust, who on Sunday, February 4th, had complained of a pain in the stomach. During that night he vomited, had a chill, and at 5 a. m. Monday, high fever. His physician was called

and at 9 a. m. found him with temperature of 100°, abdominal muscles resistant, and pain chiefly about the umbilical region. Opium and bismuth were prescribed, and later oil, which moved the bowels. The symptoms increased in severity through that night and on the next day, and on February 7th at 11 a. m., on the fourth day after the onslaught of the appendicitis, I was called to operate. The pulse was rapid and weak; temperature, 103°. Abdomen greatly distended and very painful to pressure. The facies abdominalis was present, and altogether the case seemed hopeless; nevertheless, I opened the abdomen, only to find the peritoneal cavity hyperdistended with fetid pus saturated with intestinal contents. The appendix was ruptured through an ulcerative patch, out of which ingested matter was oozing. This was removed, a second incision made in the median line, to facilitate thorough washing out of the cavity with warm boric-acid water, and drainage out of the deepest portion of the pelvis, as well as through the incision over the appendix. The child rallied fairly well. On the day following, obstructions due to paralysis of the intestine from peritonitis caused such distention that an artificial opening was made in the ileum at the wound in the iliac fossa. A large amount of gas and some ingested matter escaped and the distention was relieved. Death occurred on February 10th from septic infection.

These are two illustrative cases. One was saved by prompt operation, the other perished from delay. The death-rate in appendicitis as at present treated is about eighteen in a hundred. Richardson, of Boston, reports one hundred and eighty-one cases, forty-three deaths, or twenty-four per cent. Murphy, of Chicago, one hundred and forty-one cases, sixteen deaths, or eleven per cent. At Mount Sinai Hospital in 1893 there were treated forty-two cases with seven deaths, or 15.5 per cent., this total of three hundred and sixty-four cases giving a mortality ratio of eighteen per cent. I venture to say that if a competent surgeon were called within the first twelve hours from the first symptoms of appendicitis, this death-rate would be brought down to five per cent. or less, and I believe that if

every case were properly explored, and, if necessary, operated upon within the first twenty-four hours of the first symptom, the death-rate would not be two per cent. In Bull's statistics of three hundred and forty-one cases of chronic relapsing appendicitis, the fatal cases were seven—not quite two per cent. Granting his conclusion, that this ratio would be increased to five per cent. if all cases were made public, the conclusion must also be accepted that early operation in acute cases would be made under more favorable conditions and would yield a lower death-rate.

It may be that the surgeon who leads trumps when he is in doubt might open into the abdomen of a certain small proportion of cases in which the appendix was not diseased, but such an error is in the direction of safety. Careful and skilful exploration of the peritoneal cavity is not dangerous. The danger of ventral hernia in the line of incision is insignificant when the incision is properly made and the recumbent posture maintained for six weeks while the wound is firmly uniting.

Again, failure to operate early in the history of an appendicitis invites disaster in other forms than death. Fæcal fistula from the ruptured appendix or from the colon, which breaks down from the pressure of the abscess or inflammatory tumor; adhesions and recurrent attacks of appendicitis, compelling operation under difficult conditions; and intestinal obstruction are of frequent occurrence.

On February 13th, within a few days of the two cases first detailed, I was consulted by F. B. S., thirty years old, who came to me with his physician, Dr. Hancock, of Wisconsin. This patient had acute appendicitis in February, 1893. A large abscess was opened on the ninth day, and one week later fæcal matter escaped and had continued to escape to this date. He was subjected to operation for closure of the fistula. Had this patient been operated on within twenty-four hours of his illness, no such disaster would have followed.

On January 25th, 1894, this specimen was removed with considerable difficulty from A. W., of Kinderhook, who was brought to me by Dr. Kittel of that town. He was thirty-one years old, and in June, 1893, had a slight attack of appendicitis; a recurrence in August, lasting for thirteen days; a third seizure in September; and in December a very severe attack, which put him to bed for three weeks. At this time there was a well-marked tumor in the iliac fossa and high febrile movement. The appendix was found firmly adherent to the side of the rectum and bladder, the cæcum being greatly displaced and evidently drawn down by the weight of the inflammatory tumor and subsequent contractions. This patient made a good recovery, but he should have been operated upon in the first attack, when the danger and difficulty would have been insignificant.

On March 1st of this year, I removed this most instructive specimen from H. T., a man thirty-four years old, brought to me by Dr. Abruzzo and Dr. Bettini, of this city. A well-defined tumor was located in the right iliac fossa. Seven weeks before, he had noticed it after an attack of cramps in the bowels. It was not sensitive to pressure, nor was there any rigidity of the abdominal muscles. The temperature was normal. He said the tumor was getting larger all the time. On exposing the cæcum it was covered at its end by a tumor composed of organized lymph. Dissecting down to a depth of about three fourths of an inch into the mass, the appendix was found to form the center of the tumor. It showed the traces of an inflammatory process and looked withered up from the pressure of the enveloping lymph which had contracted around it. The tumor, with its appendix, was dissected from the cæcum, and the contiguous ileum and stump buried by inclosing sutures. At this juncture, I felt a number of hard foreign bodies in the ileum, which at once told me that there was a stricture at or near the ileocæcal valve which would allow semi-liquid intestinal contents to pass out, but arrested solid matter. I opened the ileum, introduced my index finger into the gut, and found the tip could scarcely be engaged in the opening, which was not more than a fourth of an inch in diameter. By gradual divulsion, the finger was forced through to its largest diameter and the wound in the intestine closed by

ten Lembert sutures. About two weeks later a lot of orange seeds were discharged through the anus, some of which are shown with the specimen. The patient recovered.

On April 2d, this specimen was removed from a lady about thirty-five years old, who eight days before had the first symptom of pain at the appendix. Three months before, she had experienced slight pain in this same region, which persisted mildly for three or four days and went away. In this last attack, however, the pain did not cease, but was not severe. On the fourth day, with a normal temperature, the abdomen began to swell, and for two days before I saw her she was vomiting at frequent intervals, and for twenty-four hours intestinal contents. Recognizing intestinal obstruction, I cut down upon the appendix and found it small—about a fourth of an inch thick, three inches long, and perfectly healthy, save at the tip, where a pinhole perforation existed. This had produced a general septic peritonitis and an adhesion of a loop of small intestine, with complete obstruction about four feet from the ileo-cæcal junction. The appendix was tied off and the obstruction relieved. The intestine was gangrenous at the point of occlusion and burst after it was lifted out of the peritoneal cavity. It was closed by suture and the weak spot attached to the margin of the abdominal incision. She died from sepsis thirty-six hours after the operation. If this patient had been operated upon within twenty-four hours of her second attack, she would have escaped the disaster which followed.

These cases are taken as they have come in my practice. They are the common experience of many surgeons and they point unmistakably to the wisdom of early operative interference. Given a surgeon of experience, a clean operator, who, with the minimum of traumatism to the intestines or contiguous viscera, can remove a diseased appendix, and it would be better for exploratory laparotomy to be done in every instance within the first twenty-four hours of the disease.

On the contrary, were I the subject of an attack and were I not sure of my surgeon, I would keep flat on my

back, quiet peristalsis and voluntary motion with morphine, and take the chances of resolution, encapsulation by adhesion, or rupture into the intestines. If there is one particular operation an inexperienced man should in all conscience avoid, it is this.

DISCUSSION.

DR. S. BARUCH.—As a general practitioner, it is my conviction that every case of appendicitis, providing the diagnosis is correct, is a surgical disease. This conclusion was reached by me by a gradual evolution, a narration of which may prove a lesson to others. Perhaps this can best be done by citing a few cases. In 1881, a man aged fifty-two came to my office complaining of rheumatism. The pain came on suddenly. I examined him and made a diagnosis of what we then called perityphlitis. I applied leeches and ice-bags and gave morphine, and Dr. Delafield saw the case with me in consultation. The patient died in two days of diffuse peritonitis. In June, 1883, I saw a boy aged eleven years, who complained of pain in the right iliac region. I diagnosed appendicitis, and treated him in the routine way. Dr. Loomis saw the case with me. The boy died in five days of general peritonitis. Dr. L. Weber saw the boy on the day he died, and proposed operation, but it was not accepted by Dr. Loomis, myself, or the family. In March, 1884, I saw a boy with appendicitis. Six hours after I first saw him, I called Dr. Jacobi, and while we were consulting about the case in an adjoining room, the patient died.

In consequence of the above cases, I put down in my note-book in 1884, that thereafter I should consider appendicitis a surgical disease, and that whenever I met with a case, I would call a surgeon in consultation. In February, 1888, I saw a boy aged nine years, who had appendicitis. I told the child's parents that unless he was entirely well within twenty-four hours, he must be operated on. On the following morning the boy felt better. The pain was practically gone, but I thought I saw some symptoms of collapse. Dr. Sands was called in four hours later, that is, twenty-eight hours after I first saw him, and he thought it was too late to operate. He, however, performed the operation, assisted by Dr. Hartley, and found a perforated

appendix with small concretions. That was, I believe, the first case of appendicitis ever operated on successfully.

I have had a number of cases of what I called perityphlitis, which recovered under treatment. I remember a colleague who had two such attacks in 1884, from both of which he recovered, and he is still living. Some may ask, if these patients get well without surgical interference, why should we subject them to so serious an operation as laparotomy? I have given you the histories of three cases which proved fatal, and which I think might have recovered if an early operation had been undertaken. I regard every case of appendicitis which presents plain manifestations, the same as I would a foreign body, like a bullet, which demands removal. The inflamed appendix can be regarded as nothing else than a foreign body in the abdominal cavity. In all cases of that character, I think the physician should stand in what we call among nations "armed expectancy," and be ready at any moment to advise surgical interference.

In the treatment of these cases by the physician, I think morphine should be omitted. It constipates the bowels and obscures the symptoms. When morphine has been administered in a supposed case of appendicitis, we find it very difficult to make a diagnosis. It blunts the patient's sensibility, the respiration, temperature, and pulse may be almost normal, and the consultant, even if he is a surgeon, will probably be deceived into a false sense of security. In cases where the pain is very severe, I give chloroform.

DR. S. H. DESSAU.—While I have had a very limited experience with cases of appendicitis, still, as a general practitioner, I agree in the main with Dr. Baruch. There are a certain number of cases that we diagnose as appendicitis which recover without surgical interference. It is impossible in such a case to tell what the condition of the appendix is, and some of those patients have lived for many years afterwards without a relapse. On the other hand, there are cases that relapse frequently, and these are the ones that we ought to subject to operation, either at the time of an attack, or in the interval between attacks. I think Dr. Wyeth will recall a patient he saw with me in consultation. Dr. Wyeth thought it was not necessary to operate at the time, and the patient recovered without an

operation, and for five years afterwards he had no further attack. Then he was taken ill, and when I saw him, a few hours before he died, no positive diagnosis had been made. It looked to me as much like typhoid fever as it did like appendicitis; it may have been a perforating appendicitis. I was unable to get a clear history of the case.

DR. A. M. LESSER.—It has been said here to-night that surgical interference should be advised in appendicitis, if the diagnosis can be positively made. I believe that is the great difficulty. Frequently I am called to see patients who are suffering from nothing more than an enterocolitis. In such cases pressure in the vicinity of the appendix causes pain, which is often mistaken for the pain accompanying appendicitis.

It has been my observation that the opium treatment of appendicitis has been productive of more harm than good, and I am very glad Dr. Baruch mentioned this fact. It ameliorates the symptoms, while at the same time it decidedly harms the patient. I have found a remedy to administer in these cases which not only will not harm the patient, but which, on the contrary, will benefit him, and help us to decide whether an operation is required or not. I first used it in salpingitis, in which affection I found it very serviceable. It is the salicylate of soda, which should be given in gramme doses. Pain in the vicinity of the appendix, or any pain which would cause us to fear that we have to deal with an inflammation of the peritoneum, is quickly relieved by this drug. It will not, like opium, check the secretions and increase the tympanites. It will, on the contrary, increase the secretions, reduce tympanites, relieve the pain, and sometimes check inflammation before the occurrence of gangrene. Since I have introduced the drug into my practice, I have found it easier to perform the operation. Even if perforation has taken place, the salicylate of soda seems to be the proper thing. It is valuable in any case of pelvic inflammation. It is not unreasonable to suppose that catarrhal appendicitis can be relieved without an operation as well as catarrhal salpingitis. In such cases, I give the salicylate of soda in gramme doses, every two hours.

DR. R. T. MORRIS.—This whole question is easily reduced to its principles. Appendicitis is an infective

exudative inflammation, caused by bacterial invasion of the tissues of the appendix, and persisting after subsidence of acute symptoms. Appendicitis is not at any stage a medical disease, for the reason that the bacteria which cause the infection are not killed by medical resources, although acute symptoms may be made to disappear. Surgery removes the nest of bacteria. The earlier the operation is done, the greater the credit to rational practice. A surgical death-rate of one per cent. in appendicitis is a death-rate of inexperience—inexperience on the part of patient, physician, or surgeon. A hernia-rate of one per cent. after the operation, is a hernia-rate of inexperience. Death, hernia, and suffering are preventable in appendicitis cases.

The inch-and-a-half abdominal incision which I employ keeps the patient in bed eight days. He leaves his room on the tenth day. This is practical only after the employment of a technique which unites each cut structure accurately and separately with fine catgut. It is not adapted to pus cases. Curiously enough, some surgeons have reasoned that I had easy cases to deal with, because of the results of this technique. This is not true. I have little voice in the selection of cases; these patients are sent to me by their family physicians, who send cases that they believe to be in need of operation—many of them after desperate complications. There is no theoretical feature about this technique that I employ. It is a practical, every-day, routine work in which I am engaged. Appendicitis cases cannot be medical cases, because of their nature, but I depend upon the general practitioner to manage the cases apart from my special work, and should feel very uncomfortable were I to be deprived of his counsel and aid. I do not come across any cases of catarrhal appendicitis.

DR. J. A. WYETH.—It appears to me that the salicylate of soda would prove absolutely impotent in correcting any septic process that may involve the peritoneum. I have had no experience with the drug in these cases. I am afraid that like many other remedies that have been recommended, it would simply lead to delay in calling the surgeon and thus keep up the mortality rate.

In regard to Dr. Morris's inch-and-a-half incision. While I have great admiration for his skill, I should not like to

take the risk of removing the appendix through such a small incision. I do not believe I could do it. I would not do it on anybody, and I would not allow anybody to do it on me. While working in the dark, the surgeon might cause perforation or break through the wall of an abscess, and thus infect the entire peritoneal cavity. It is much better, I think, to make a free incision, so that one can see what one is doing and introduce gauze to shut off the diseased area. I do not specify any definite period of time for the patient to remain in bed. I keep my patients quiet for about six weeks, so that the wound may become well knitted, and then I think there is no fear of a ventral hernia. In none of my cases has a hernia developed.

I think that to Dr. Baruch, more than to any other one man, belongs the credit of having impressed on the general profession the fact that surgery is of the utmost importance in the successful treatment of appendicitis.

Stated Meeting, April 19th, 1894.

PERSISTENT ALBUMINURIA AND GLYCOSURIA, WITH FREQUENT HYALINE CASTS, IN FUNCTIONAL NERVOUS DISEASES.

By LANDON CARTER GRAY, M. D.

For many years, in the fear that I might overlook some lurking renal trouble, I have been in the habit of having examinations of the urine made in cases of neurasthenia and other cases of functional disease that have sought my advice; and especially, the difficulty of treatment of many of the neurasthenics has led me, within the past two years, to very careful testing with the hope that I might therefrom derive indications for a rational therapeusis. To this end I have been fortunate in enlisting the services of Mr. Albert Zimmerman, in charge of the laboratory of Mr. Horatio N. Fraser, who has entered with great enthusiasm upon the task, and spared no pains in details that have often been wearisome in the extreme. In this way there have been examined thirty-one cases of neurasthenia, one case of subacute mania, two of Raynaud's disease, two of hypochondria, one of *folie de doute*, or mysophobia, five of melancholia, four of vertigo, one of lumbago, and for reasons that will be seen farther on, one case of diabetes mellitus, one of spinal syphilis, and three of hemiplegia. In all of these cases tests have been made for albumin, sugar, urea, the phosphates, uric acid, calcium oxalate, casts, and indican; and in many of them the urine has been tested for nucleo-albumin from the bile, for serum-globin, fibrinogen, urobilin, and bile.

Of the thirty-one cases of neurasthenia the following were the results:

In cases of one examination, heavy traces of albumin	13
" " " " faint " " "	14

In cases of more than one examination:

In one	albumin was first	faint,	then heavy,	then none.	
In another	" "	heavy,	" faint,	" heavy.	
" "	" "	faint,	" faint.		
" "	" "	heavy,	" heavy.		
" "	" "	heavy,	" faint,	" none.	
" "	" "	faint,	" faint.		
" "	" "	faint,	" faint.		
" "	" "	faint,	" faint.		

In one case there was no albumin.

Albumin from bile . . .	6	Fibrinogen	2

Of these thirty-one cases of neurasthenia, the sugar was about:

0.10 per cent. in	16	0.05 per cent. in	8
0.15 " "	7	0.03 " "	1

In the cases where there were heavy traces of albumin, the urea and the specific gravity contrasted as follows:

Urea.	Specific gravity.	Urea.	Specific gravity.
3.24	1.024	2.106	1.027
2.808	1.026	1.917	1.028
1.728	1.017	1.998	1.032
2.052	1.027	1.836	1.022
3.186	1.024	2.376	1.028
1.674	1.018	1.998	1.026
1.89	1.031		

In cases with faint traces of albumin, the urea and the specific gravity were as follows:

Urea.	Specific gravity.	Urea.	Specific gravity.
1.782	1.026	1.836	1.024
2.538	1.029	1.647	1.029
1.782	1.024	2.025	1.028
2.214	1.022	1.809	1.026
1.566	1.016	2.376	1.022
1.836	1.021	4.320	1.029
1.674	1.019	3.888	1.027
2.376	1.029	2.808	1.024
1.836	1.022	3.564	1.031
2.106	1.027	3.078	1.030
1.62	1.017	2.592	1.030
1.566	1.016		

In the forty-four examinations of urine, in the thirty-one cases of neurasthenia, the uric acid, oxalate of calcium, phosphates, indican, and casts were:

Phosphates above normal	33	Casts, present	20
Normal	11	absent	24
Uric acid, above normal	44	Indican, above normal	43
Calcium oxalate, above normal	44	normal	1

Of these forty-four examinations, twenty-one were of one case only. In three cases, three specimens were examined. In seven cases, two specimens were examined.

In five cases of melancholia, one examination each, there were:

Albumin, heavy traces	2	Urea and specific gravity contrasted:	
faint traces	3	faint tr. alb. 2.365 sp. gr. 1.025	
from bile	2	2.052 1.024	
Sugar, 0.10 per cent.	2	Calcium oxalate, normal	1
0.05 "	3	above normal	4
Urea and specific gravity contrasted:		Uric acid, above normal	4
heavy tr. alb. 2.592 sp. gr. 1.024		normal	1
2.00 1.030		Casts, few hyaline	4
faint tr. alb. 0.702 1.012		absent	1
		Indican, above normal	5

In two cases of hypochondria, four specimens examined, there were:

Albumin, heavy traces	1	Phosphates, above normal	4
faint traces	2	Uric acid, above normal	4
none	1	Calcium oxalate, above normal	4
Sugar, 0.10 per cent.	2	Casts, few hyaline	3
0.05 "	1	none	1
0.075 "	1		
Urea and specific gravity contrasted:			
faint tr. alb. 2.10 sp. gr. 1.029		heavy tr. alb. 1.89 sp. gr. 1.030	
1.188 1.027		none 1.975 1.029	

In one case of *folie de doute*, four specimens examined, there were:

Albumin, faint traces	3	Phosphates, normal	1
none	1	above normal	3
Albumin, from bile	1	Uric acid, normal	1
Sugar, 0.10 per cent.	3	above normal	3
0.05 "	1	Calcium oxalate, normal	1

Urea and specific gravity contrasted:		Calcium oxalate, above normal	3
faint tr. alb. 3.672	sp. gr. 1.026	Indican, above normal	4
3.78	1.027	Urobilin, slight	2
2.70	1.023	absent	2
no albumin, 3.726	1.025		

In two cases of Raynaud's disease, one specimen each, there were:

Albumin, heavy traces	1	Phosphates, normal	1
faint traces	1	above normal	1
fibrinogen	1	Uric acid, above normal	2
Sugar, 0.10 per cent.	1	Calcium oxalate, above normal	2
0.03 "	1	Urobilin, slight	1
Urea and specific gravity contrasted:		none	1
heavy tr. alb. 2.268	sp. gr. 1.022	Casts, few hyaline	1
faint tr. alb. 2.86	1.022	none	1

In three cases of insomnia, one specimen each examined, there were:

Albumin, heavy traces	1	Phosphates, normal	1
faint traces	2	above normal	2
from bile	1	Uric acid, above normal	3
fibrinogen	1	Calcium oxalate, above normal	3
Sugar, 0.05 per cent.	3	Casts, few hyaline	2
Urea and specific gravity contrasted:		absent	1
heavy tr. alb. 1.944	sp. gr. 1.024	Indican, above normal	3
faint tr. alb. 1.134	1.018	Urobilin, slight	1
1.458	1.010	absent	2

In one case of *petit mal*, one specimen examined, there were:

Serum-globin, faint traces	1	Phosphates, normal	1
Sugar, 0.10 per cent.	1	Uric acid, above normal	1
Urea and specific gravity contrasted:		Calcium oxalate, above normal	1
urea, 0.702	sp. gr. 1.009	Indican, normal	1
		Casts, absent	1

In two cases of epilepsy, one specimen each examined, there were:

Albumin, heavy traces	1	Phosphates, normal	2
faint traces	1	Uric acid, above normal	2
serum-globin	1	Calcium oxalate, above normal	2
Sugar, 0.02 per cent.	1	Indican, above normal	2
absent	1	Casts, few hyaline	1
Urea and specific gravity contrasted:		absent	1
heavy tr. alb. 1.674	sp. gr. 1.017		
faint tr. alb. 1.242	1.012		

In one case of post-epilepsy, one specimen examined, there were:

Albumin, faint traces	1	Uric acid, above normal	1
Sugar, 0.05 per cent.	1	Calcium oxalate, above normal	1
Urea and specific gravity contrasted:		Indican, normal	1
Urea, 1.836 sp. gr. 1.019		Casts, absent.	
Phosphates, normal	1		

In four cases of vertigo, six examinations of urine; three in one, one each in three, there were:

Albumin, heavy traces	3	Phosphates, normal	2
faint traces	3	above normal	4
from bile	5	Uric acid, above normal	6
Sugar, 0.10 per cent.	5	Calcium oxalate, above normal	6
0.05 "	1	Casts, few hyaline	4
Urea and specific gravity contrasted:		absent	2
heavy tr. alb. 3.024 sp. gr. 1.029		Indican, normal	1
3.294 1.028		above normal	5
1.944 1.025		Urobilin, slight	4
faint tr. alb. 1.674 1.029		absent	2
2.16 1.027			
2.025 1.020			

In one case of lumbago, four examinations of urine, there were:

Albumin, faint traces	2	Phosphates, normal	1
heavy traces	2	above normal	3
from bile, faint traces	2	Uric acid, above normal	4
Sugar, 0.10 per cent.	2	Calcium oxalate, above normal	4
0.15 "	2	Indican, above normal	4
Urea and specific gravity contrasted:		Urobilin, slight	4
heavy tr. alb. 2.052 sp. gr. 1.025		Casts, few hyaline	4
2.16 1.026			
faint tr. alb. 2.322 sp. gr. 1.028			
3.078 1.030			

In one case of neuritis, one specimen examined, there were:

Albumin, faint traces	1	Uric acid, normal	1
Sugar, 0.01 per cent.	1	Calcium oxalate, normal	1
Urea and specific gravity contrasted:		Casts, absent	1
urea, 1.296 sp. gr. 1.012		Indican, normal	1
Phosphates, normal	1		

In one case of post-neuritis, one specimen examined, there were:

Albumin, heavy traces	1	Phosphates, normal	1
fibrinogen	1	Uric acid, normal	1
Sugar, 0.02 per cent.	1	Calcium oxalate, normal	1
Urea and specific gravity contrasted:		Indican, normal	1
urea, 1.728 sp. gr. 1.014		Casts, absent.	

In one case of rheumatic neuritis, one specimen examined, there were:

Albumin, faint traces	1	Phosphates, normal	1
Sugar, 0.05 per cent.	1	Uric acid, above normal	1
Urea and specific gravity contrasted:		Casts, absent.	
urea, 1.296 sp. gr. 1.014		Indican, above normal	1
Calcium oxalate, above normal	1		

In one case of hysteria, one specimen examined, there were:

Albumin, faint traces	1	Calcium oxalate, above normal	1
Sugar, 0.02 per cent.	1	Phosphates, above normal	1
Urea and specific gravity contrasted:		Indican, above normal	1
urea, 2.268 sp. gr. 1.022		Urobilin, slight	1
Uric acid, above normal	1	Casts, absent.	

In one case of nymphomania, one specimen examined, there were:

Albumin, faint traces	1	Uric acid, above normal	1
Sugar, 0.10 per cent.	1	Calcium oxalate, above normal	1
Urea and specific gravity contrasted:		Indican, above normal	1
urea, 2.025 sp. gr. 1.020		Casts, few hyaline	1
Phosphates, normal	1		

In one case of neuralgia, one specimen examined, there were:

Albumin, very faint traces	1	Phosphates, above normal	1
Serum-globin, very faint traces	1	Uric acid, above normal	1
Sugar, 0.15 per cent.	1	Calcium oxalate, above normal	1
Urea and specific gravity contrasted:		Indican, above normal	1
urea, 3.537 sp. gr. 1.025		Casts, few hyaline	1

In one case of torticollis, one specimen examined, there were:

Albumin, heavy traces	1	Uric acid, above normal	1
Sugar, 0.05 per cent.	1	Calcium oxalate, above normal	1
Urea and specific gravity contrasted:		Indicin, abundant	1
urea, 1.728 sp. gr. 1.020		Casts, few hyaline	1
Phosphates, normal	1		

In one case of tic-douloureux, one specimen examined, there were:

Albumin, faint traces	1	Uric acid, above normal	1
Sugar, 0.02 per cent.	1	Calcium oxalate, above normal	1
Urea and specific gravity contrasted:		Indican, above normal	1
urea, 1.566 sp. gr. 1.010		Urobilin, absent	1
Phosphates, above normal	1	Casts, absent	1

In one case of intra-cranial syphilis, one specimen examined, there were:

Albumin, heavy traces	1	Uric acid, above normal	1
Sugar, 0.15 per cent.	1	Calcium oxalate, above normal	1
Urea and specific gravity contrasted:		Indican, above normal	1
urea, 2.214 sp. gr. 1.020		Casts, absent.	
Phosphates, above normal	1		

In two cases of spinal syphilis, one specimen each examined, there were:

Albumin, heavy traces	1	Phosphates, above normal	1
faint traces	1	normal	1
from bile, heavy traces	1	Uric acid, above normal	2
Sugar, 0.05 per cent.	2	Calcium oxalate, above normal	2
Urea and specific gravity contrasted:		Casts, abundant, hyaline	1
faint tr. alb. 1.782 sp. gr. 1.029		absent	1
heavy tr. alb. 1.404 1.028		Indican, above normal	2
		Urobilin, slight	2

In one case of nervous syphilis, two specimens examined, there were:

Albumin, heavy traces	2	Phosphates, normal	2
fibrinogen	1	Uric acid, above normal	2
Sugar, 0.10 per cent.	1	Calcium oxalate, above normal	2
0.05 "	1	Indican, above normal	2
Urea and specific gravity contrasted:		Urobilin, absent	2
urea, 2.376 sp. gr. 1.019		Casts, few hyaline	2
1.82 1.017			

In one case of alcoholic meningitis, six specimens examined, there were:

Albumin, abundant	1	Phosphates, normal	3
faint traces	4	above normal	3
none	1	Uric acid, above normal	5
Sugar, 0.05 per cent.	2	normal	1
0.03 "	4	Calcium oxalate, above normal	5
Urea and specific gravity contrasted:		normal	1
heavy tr. alb. 2.025 sp. gr. 1.025		Indican, above normal	5
faint tr. alb. 1.512 1.022		normal	1
1.674 1.017		Casts, abundant hyaline	1
1.728 1.021		few hyaline	2
1.620 1.019		absent	3
no albumin 2.279 1.019			

In one case of general paresis, one specimen examined, there were:

Albumin, faint traces	1	Uric acid, above normal	1
Sugar, 0.03 per cent.	1	Calcium oxalate, above normal	1
Urea and specific gravity contrasted:		Indican, abundant	1
urea, 1.458 sp. gr. 1.014		Urobilin, absent.	
Phosphates, normal	1	Casts, few hyaline	1

In three cases of hemiplegia, one specimen each examined, there were:

Albumin, faint traces	3	Phosphates, above normal	2
from bile, heavy traces	1	Uric acid, above normal	3
Sugar, 0.10 per cent.	2	Calcium oxalate, above normal	3
0.075 "	1	Casts, few hyaline	2
Urea and specific gravity contrasted:		absent	1
urea, 1.296 sp. gr. 1.014		Indican, above normal	3
2.484 1.031		Urobilin, absent	2
2.268 1.023		slight	1
Phosphates, normal	1		

In one case of subacute mania, two specimens examined, there were:

Albumin, faint traces	2	Uric acid, above normal	2
from bile, heavy traces	1	Phosphates, above normal	2
Sugar, 0.15 per cent.	1	Calcium oxalate, above normal	2
0.10 "	1	Indican, abundant	2
Urea and specific gravity contrasted:		Urobilin, slight	2
urea, 2.754 sp. gr. 1.026		Casts, few hyaline	1
2.916 1.028		absent	1

In one case of diabetes mellitus, five specimens analyzed, there were:

Albumin, faint traces	4	Phosphates, above normal	5
none	1	Calcium oxalate, above normal	5
Sugar, 0.10 per cent.	2	Uric acid, above normal	5
0.05 "	3	Casts, none	5
Urea and specific gravity contrasted:		Indican, above normal	5

faint tr. alb.	1.94	sp. gr. 1.030
	2.268	1.031
	1.944	1.031
	2.16	1.032
no albumin	2.118	1.029

In most of these cases, therefore, albumin, sugar, excess of phosphates, oxalate of calcium, uric acid, and indican have been found.

How are we to interpret these findings in the light of our present knowledge of the genesis of these different constituents of the urine?

The almost constant presence of albumin in heavy or slight traces has been of the most interest to me. It is a well-recognized fact that this substance is often to be found in the urine of normal individuals under certain well-known conditions. This has been shown by Jackson[1] to occur after cold baths; in the newborn[2]; in children and young people[3]; in pregnant and nursing women[4]; after muscular exertion and perspiration, and after meals. It is also well known that it occurs in many pathological conditions other than in nephritis, such as lipuria[5], chyluria, hæmoglobinuria, and from compression of the thorax[6]. None of these causes have been present in my cases. I have, moreover, been especially careful in all these patients to look for the cardiac and arterial lesions characteristic of nephritis, and in many of them I have also tested the arterial pressure, and whenever I have found

[1] British Medical Journal, 1873.
[2] Virchow's Ges. Abhndlng. 1856.
[3] Monat. f. Geburtsh, Bd. xxix-p. 105.
[4] Zeitschr. f. Geb u. Frauenkrkh., 1876-i-273.
[5] Ribbert: Virchow's Arch., 1884-xcviii-527.
[6] Schreiber: Arch. f. exp. Pathol., 1885-xix-237.

any of the former they have been excluded from these tables, whilst I have admitted some few instances of the latter who presented no other evidences of renal lesion. My tables may therefore be taken to consist of cases in which no nephritis was evident, except one choose to regard the albuminuria as such. I am very loath, however, to jump to this latter conclusion, because of the fact that the course of these cases is not that of any form of nephritis, as most of them are perfectly curable, as well as for the reason that the same amount of albuminuria is found in such other cases of functional nervous disease as I have catalogued. In other words: Either neurasthenia, *folie de doute*, hypochondria, melancholia, vertigo, with slight albuminuria, are cases of nephritis, or else slight albuminuria is merely an attendant symptom of these cases of functional nervous disease. The former proposition seems to me ridiculous, inasmuch as there is not the slightest proof of it, whilst the latter belief is at least tenable, because all the proofs so far attainable are in favor of it. Moreover, these cases of albuminuria are almost always attended by varying amounts of urinary sugar, which is not necessarily a concomitant symptom of nephritis. On the other hand, I cannot remember ever having seen a case of albuminuria in functional nervous disease pass into well-marked nephritis. Of course, this statement must be taken with some reservation, because, as I have already said, I have only had the urine of these cases thoroughly examined within the last two years; but then, I have treated many hundreds of such cases of neurasthenia, and I think it is probable that my memory would not have deceived me had any of them ever passed on into any form of renal lesion. So far as I have seen, I believe that the renal diseases begin to show the arterial or cardiac symptoms very soon after the albuminuria is manifested, or indeed occasionally even before, whilst œdema is a frequent symptom when the disease has lasted for some time.

In these particulars, therefore, the albuminuria which I am describing would seem to be of a different type of disease. For the last thirty years, Semmola, of Naples, has been claiming that the albumin found in the urine in Bright's disease is a different kind of albumin from that which is found in other lesions, as, for instance, those of the heart; nor, from *a priori* reasoning and the facts which I have adduced, does this seem at all improbable. Indeed, in one of my cases I found that the albumin was albumose, and, as I have already said, such other albumins as fibrinogen and albumin from bile have been frequently encountered. It would be very tempting, indeed, for me here to pass into a digression on the researches of Meissner, Brücke, Schützenberger, Kühne, Neumeister, Chittenden, and others, as to the action of the gastric juice and its constituents on the proteids, and to discuss with you what we know about parapeptone, metapeptone, hemiprotein, hemialbumin, antialbumid, antialbumat, antialbumose, hemialbumose, antipeptone, and all the varieties of the albumoses; but, aside from the fact that this would constitute a paper in itself, these distinctions are of no pertinency to this investigation except to the chemical investigator himself. I have only been able to obtain two communications of Semmola,[1] presented to the Academy of Medicine of Paris respectively in 1883 and 1887. In these two papers are contained a series of experiments upon dogs, showing that forced alimentation with the white of egg produced first a congestion and then hæmorrhage between and within the glomeruli, as well as between the glomerulus and Bowman's capsule; and, further on, a diapedesis of leucocytes upon the smaller tubes and between the uriniferous tubes, together with alteration of the epithelium of the tubes between the ascending loop of

[1] Nouvelles contributions à la pathologie et au traitement de la maladie de Bright. L. Di Mariano Semmola, Paris, 1883. Nouvelles recherches expérimentales cliniques sur la maladie de Bright, Paris, 1887.

Henle and the straight tubes; whilst with still larger quantities of white of egg, molecular alterations extended throughout all the structures of the kidney. This is conclusive so far as it goes, but it only shows the effect of one form of albumin—that of the white of egg; but Stockvis has found that even this white of egg does not appear in the urine when it has been cooked before ingestion. Whether other kinds of albumin are capable of causing this implication of the kidney is a matter as yet unsolved, and these experiments, therefore, cannot be applied to the determination of the effect of the albumin which I have found in my cases, as this was certainly not an egg albumin. It is a significant fact in this connection that, until within the last ten years, none of the life insurance companies of Continental Europe have been in the habit of examining the urine of applicants; and yet their death-rates, so far as statistics are capable of showing, do not differ from those of the American companies, in which the urine is always carefully examined.

I have found Millard's test to be a much more reliable one than that by heat and nitric acid, inasmuch as I have frequently failed to find slight amounts of albumin by the latter when they have been found by the former; whilst, on the other hand, I have never failed to find albumin with heat and nitric acid when it had been demonstrated by Millard's test. It has frequently happened to me to have the urine of patients examined by excellent physicians without the finding of albumin, and yet I have been able to demonstrate it to myself conclusively; indeed, so often has this happened that I have grown to be very skeptical about the ordinary medical examination of urine. The explanation of this discrepancy to my mind lies in the fact that perfectly demonstrable slight or heavy traces of albumin can very readily be overlooked unless the test-tube is sparklingly clean and held in a proper light. I think, too, that we physicians have been so much accustomed to

finding great flakes of albumin, or coagulated or heavy clouds, such as we see in advanced cases of nephritis, that we have grown to be careless in the determination of relatively smaller amounts. Nevertheless, albumin is albumin, whether it fills the half of a test-tube or causes mere streaking that is only visible to the eye of a careful examiner. I have also asked myself whether, with such a delicate test as Millard's, albumin would not be found in every urine, and the more especially as Posner,[1] examining the urine of seventy healthy persons, found albumin almost constantly, although doubt, it should be borne in mind, has been cast upon these observations by Malfatti,[2] who shows that what was mistaken for serum-albumin was really mucin, coming from the mucous glands of the urinary passages. In about 20 per cent. of the cases, however, of various kinds which I have carefully examined with Millard's test, no albumin whatever has been found, whilst I can count up over a thousand cases that have been examined by other physicians by Millard's test, in which albumin was not found either, although, for reasons which I have detailed already, I should not insist upon the absolute accuracy of these latter examinations. In 1,017 cases in which the urine was examined in applicants for life insurance, the results were as follows:

No albumin	863
Albumin found by heat test	24
" " " Heller's No. 3	27
" " " Tanret's test	43
" " " Millard's test	45
" " " picric acid	5
Total number in which albumin was found	144

The sugar which I have found in these urines has been determined by either Fehling's or the indigo carmine test. I have found the latter far the most delicate and reliable, and I now use it in all cases. That there is some close rela-

[1] Berlin. klin. Wochenschr., 1885-xxii-654.

[2] Internat. Centralbl. f. Phys. u. Path. d. Harnorg., 1889.

tionship between glycosuria and these cases attended with albumin and nephritis on the one hand, and true diabetes on the other, has seemed probable to me, from the following facts: First, a young man coming to me from a far Southern state had all the facial appearances of a case of diabetes mellitus, as well as the great thirst and the marked odor of the breath. Four examinations of the urine, at different times, showed heavy traces of sugar. I ordered him to go to a Western climate, whence he returned to me after a year's interval, and then I found that the thirst and the odor of the breath had disappeared entirely, whilst the sugar and albumin had vanished from the urine. Second, several of these individuals have presented the blanched *facies*, characteristic of renal disease just before the time when œdema of the lids is prone to appear.

These cases which I am describing are probably the cases which have postured for many years in medical literature as those of lithæmia. In other words, I believe that most, if not all, cases in which there is an excess of uric acid in the urine will be shown to present traces of albumin and sugar, provided the patient has shown symptoms of ill health. I am not prepared to say, however, that excess of uric acid alone will cause ill health, or that there may not be cases of excess of uric acid in the urine without albumin and sugar. The whole question of the formation and the import of uric acid seems to me to be as far from solution as ever. The two great theories to-day are those of Haig and Sir William Roberts, and they are diametrically opposed. Haig,[1] who has written so charming a book on the rôle of uric acid in disease that one is really disappointed in finding any logical objection to his conclusions, maintains that he has estimated for six years the uric acid which he himself excreted, and that the relation of uric acid to urea is about 1 to .305, varying to 1 to 0.35 or even 1 to 0.38, the mean being about 1 to 0.33. He

[1] A. Haig: Uric acid as a factor in the causation of disease. 1892.

also quotes Yvon and Berlioz as finding that the relation was about 1 to 0.30 or 1 to 0.40, whilst Lecanu (this is quoted from Sir Dyce Duckworth, *A Treatise on Gout*, p. 20) found the relation to be 1 to 0.33. This uric acid, however, is excreted in varying quantities from day to day and hour to hour, and Haig explains this by the alternation of acidity and alkalinity of the urine, accompanied, he claims, by similar acid and alkaline conditions of the blood. Thus, when the urine and blood are alkaline the uric acid is held in solution and dissolved out of the various tissues of the body, whilst acidity of the urine and blood drives the uric acid back into the different tissues. As Garrod has shown that there is a so-called acid and alkaline tide of the urine, the alkalinity being greater during the day and the early morning, and the acid at its highest during the afternoon and at night, Haig attempts to show that the early day and the morning are the times when the uric acid is held in solution in the blood and urine, and, passing through the system, causes all the ill symptoms incident to such a condition, whilst comparative ease and comfort are obtained in the afternoon and evening, as the blood becomes more acid and the uric acid no longer circulates throughout the system. Upon these explanations, Haig bases a number of clinical and pathological conclusions. Thus, if the excretion of uric acid falls below what he considers the average amount, the surplus must be hidden away somewhere in the tissues, and this surplus can be brought again into the blood and thence into the urine by drugs or by certain febrile, dietetic, or medicinal means. Haig himself has been a lifelong sufferer from migraine, and he believes that he has obtained very great relief by putting himself upon a diet consisting of milk, eggs, fish, fowl, game, vegetables, fruits, and tea, coffee, or cocoa in moderation. He claims, moreover, that the medicinal substances increasing the excretion of uric acid are alkalies, salicylic acid and its

compounds, salicin, salol, and other compounds of this class, such as phosphate of soda, piperazidin, and quinine, whilst the chief substances diminishing the excretion of uric acid are acids, iron, lead, lithia, manganese, calcium chloride, acid phosphate of soda, some sulphates, chlorides, etc., and many substances which directly or indirectly cause the acidity or otherwise form insoluble compounds with uric acid, as opium, cocaine, mercury, antipyrine, caffein, the nitrites, some hyposulphites, and strychnine. The most valuable of the excretants he considers to be the alkalies, salicylic acid and its compound form, salicyluric acid. He points out that phosphate of soda, although a valuable excretant, can only act in an alkaline medium, or while the supply of alkalies is abundant, as the presence of any acid or even of a salt of a mineral acid, as a sulphate, appears to convert it into the acid phosphate when there is no longer a plus excretion of uric acid—so that the phosphate of soda should be given with alkalies.

Salicylates, on the other hand, act best when the alkalinity is low, and their action as regards the excretion of uric acid appears to be absolutely hindered by the presence of alkali, so that they do best in conditions of fever, when the alkalinity is generally low, and in conditions where there is little or no fever it may be necessary to give them with opium or ammonia, which raise the acidity, or in alternate doses with acids to obtain their full effect on the excretion of uric acid. Piperazidin appears to have little effect upon the excretion of uric acid, and is also impaired in its effects by acids. Upon this basis of commingled facts and theories, Haig blazes a path into many obscure phenomena of disease, very much to his own satisfaction. Unfortunately, however, many of his arguments are pure theory, and of his clinical data, although I have tried to verify several of them, there is only one which has been confirmed by my own experience, namely,

that a strict observance of his theory about the action of drugs will do more to modify the suffering of migraine than any therapeutic procedure which I have tried in a medical life of twenty-one years. I have found that case after case of migraine will be benefited by the administration of twenty drops of nitro-muriatic acid before meals, and three to five grains of the salicylate of soda after meals; and modified, too, in such a way that the attacks of migraine have in several instances disappeared for months, and even then, returning, are very much less severe than formerly. His restriction of the diet I have frequently tried in former years without the slightest effect, although my experiments at that time were open to the objection that I was not acquainted with his medicinal hints; but I have not been willing to try it again with my patients, because restriction of nitrogenous diet is, in my experience, a very dangerous thing for the man or woman who has this world's work to do, inasmuch as it diminishes their well-being seriously, and thus limits their output of energy.

Haig himself admits that there is some force in this objection, because he especially cautions against a sudden reduction of nitrogenous food. I might have been willing to try again the more gradual and systematic reduction of nitrogenous material as food, had not the examinations, which I am describing to-night, of the urines of different patients caused me to think that in all these cases of mine, and possibly even in Haig's, we had to deal with an increased excretion not only of uric acid, representing one of the terminal forms of nitrogenous diet, but also with albumin and sugar, representing respectively another nitrogenous excretion, as well as one of the hydrocarbons.

Moreover, in the "Croonian Lectures" for 1892, Sir William Roberts has a theory of uric acid which is diametrically opposed to Haig's, and the one is quite as well supported as the other by seeming facts. Let me ask

your attention for a few minutes to Sir William Roberts's almost revolutionary propositions. As is well known, uric acid is regarded by chemists as bi-basic, *i. e.*, as an acid containing two atoms of replaceable hydrogen represented by the formula $H_2 (C_5 H_2 N_4 O_3)$, or more simply as $H_2 u$. It forms, like every bi-basic acid, two regular areas of cells, namely, neutral urates with the $M_2 u$, and acid urates or bi-urates with the general form MHu. The neutral urates cannot exist in the living organism, as they can only be produced in the presence of caustic alkalies and in the absence of carbonic acid and the carbonates, and Sir William Roberts therefore dismisses them from consideration. The bi-urates, on the other hand, are sparingly soluble in water, but are not decomposed thereby; and when such solutions are evaporated, the bi-urates are again deposited unchanged. They are never seen as a deposit in the unchanged urine, either in health or disease; but when the urine undergoes ammoniacal decomposition, the ammonium urate may occasionally be detected in the sediment as minute elongated dumb-bells, mixed with the amorphous phosphate of lime and the ammonia-magnesian phosphate, which constitutes the ordinary sediment of decomposed urines. Although these bi-urates are salts of uric acid, Sir William Roberts contends that there is no proof that uric acid ever exists as a true bi-urate in solution in the normal urine, and he introduces evidence to show that when these bi-urates are artificially introduced into the normal urine they fail to maintain their integrity, and at once undergo a change of composition; nor does he deem that there is any actual proof that uric acid ever exists as a true bi-urate in the healthy blood and interstitial juices. Nevertheless, it is susceptible of proof, in his opinion, that in certain abnormal circumstances bi-urates can and do arise under conditions which habitually prevail in the living body; thus, artificial solution of uric acid in healthy blood serum will

ultimately give rise to the production of crystals of sodium bi-urate.

The word "ultimately" should here be emphasized, because uric acid does not at first enter into solution in blood serum as true bi-urate, but this state of combination is slowly developed and gradually obtained after a certain lapse of time. He admits, however, that uric acid in some form of combination or other does exist normally in minute quantities in healthy blood, and he attempts to solve the question as to what this form of combination is. First, he demonstrates the identity of the amorphous, solid, or semi-solid urinary deposit with the urinary secretion of birds and serpents. He calls attention to the researches, away back in 1862, of Dr. Bence Jones, who had shown that the action of water on the amorphous urate deposit and kindred compounds, is to split them up into free uric acid and the bi-urates contained in them; but if this theory were correct, the quantity of uric acid thrown out by water should, of course, exactly equal the quantity retained in the solution as bi-urate. This was precisely what Bence Jones failed to demonstrate.

Continuing the investigation onward from this point, Sir William Roberts has apparently proved that there exists a third order of uric acid salts differing essentially from the two urates already known, and having a composition corresponding to the hypothetical formula of a quadri-urate, the form of which would be $H_2 (C_5 H_2 N_4 O_3) MH (C_5 H_2 N_4 O_3)$, or, he says more simply, $H_2 u$, MHu. These quadri-urates present themselves usually as amorphous powders, but the spheres of birds' and serpents' urine are distinctly crystalline, and display a black cross. These forms are permanent if kept perfectly dry; they readily assume a gelatinous modification, and when examined under the microscope in this state appear as large, translucent globules. They are extremely unstable, and they tend to change in two opposite directions. In weak

solutions of the alkaline carbonates or the bi-metallic phosphates they slowly take up an additional atom of the base and are converted into bi-urates. On the other hand, in water and in watery solutions of the neutral salts they are split up into free uric acid and bi-urates.

These two actions cover the behavior of the quadri-urates in the blood and the urine respectively, and furnish a key to the chemical processes which culminate in the formation of gouty deposits and calculous concretions. The only proper solvent for the quadri-urates is the healthy urine. In acid urines they dissolve freely with the aid of heat, and are again precipitated unchanged on cooling. Such solutions are, however, not stable; after a time their uric acid is slowly and at length completely liberated. The quadri-urates are still more freely soluble in hot alkaline urines, and in these media they continue perfectly unaltered and guarded against septic changes. When such solutions are made at boiling heat, they throw down, on cooling, bulky deposits which are identical with the natural amorphous urate sediment.

From these experiments, therefore, Sir William Roberts draws the conclusions: first, that free uric acid is not known physiologically either in the body or the urine, but clinically and pathologically as crystalline sediments in the urine and as gravel and calculus in the urinary passages; second, that the neutral urates are not known physiologically or pathologically, but only as laboratory products; third, that the bi-urates are known pathologically as components of gouty concretions in the tissues, and only in the urine after the secretion has undergone ammoniacal fermentation, whilst it is doubtful if they ever exist physiologically in the blood or tissues; that the quadri-urates are especially the physiological combinations of uric acid, existing normally in the urine and probably also in the blood—the latter constitute the entirety of the urinary secretion of birds and serpents—and

all the morbid phenomena due to uric acid arise probably from secondary changes in the quadri-urates. He adds to this that "These considerations point to the idea that the residuum of uric acid in mammalian urine may be something in the nature of a vestigial feature—something analogous with the vermiform appendix, the ductus arteriosus, or the ear point. These residuary structures are now regarded by biologists as indications of the line of ontological descent. On this view, the presence of uric acid in the mammal's urine should be regarded as a memory of some ancestral form which eliminated its nitrogens as uric acid. The extreme inconstancy of uric acid, both in regard to its presence and proportion in the urine of different species, and in different individuals of the same species, agrees perfectly with this theory, for vestigial structures are remarkable for their inconstancies." This author is inclined to regard the fact that uric acid (gravel) is constantly seen as an independent disorder, without disease of the stomach or liver, or general constitutional vice, in persons who are in all other respects perfectly healthy, as a vestigial or atavistic relic.

Sir William Roberts also considers the effect of the precipitation of uric acid in the urine, of the degree of urinary pigmentation, the proportion of salines, the percentage of uric acid, and, above all, the grade of the acidity of the urine; but I have not space enough at my command to do more than to say briefly that he considers the deficiency of pigment as undoubtedly a factor, but is not able to offer any therapeutic suggestions to meet it; that as to the saline matters, he advises that the patient's food contain an adequate proportion of articles rich in such substances, *i. e.*, meat, fish, eggs, and salads; that he regards excessive acidity of the urine as an important factor in the precipitation of uric acid, most likely to occur during the time of sleep, and advises for this purpose the administration of the citrate or bitartrate of potash and

soda, the former being the best preparation, to be administered at bedtime, forty to sixty grains dissolved in three or four ounces of water. He moreover believes that, as each meal acts on the urine as a dose of alkali, frequent meals are desirable. He deprecates the use of mineral waters of any kind as being no more efficient than equivalent quantities of distilled water would be.

Thus, as you see, we are met by two widely differing theories. Haig supposes that uric acid is formed day by day in the blood and thence passes into the urine, whilst Roberts maintains that the uric acid is held in solution almost entirely as a quadri-urate and is only precipitated as uric acid in the kidneys, the ureters, or the bladder, at the same time that the constant amount of uric acid in the blood is very small and is only a vestigial remnant. The former assigns to uric acid a large primary rôle, whilst the latter regards it as a factor which only becomes important in certain conditions, inducing changes localized in the peripheral parts of the urinary apparatus. When these differences of opinion between two competent observers are taken into consideration, together with the albuminuria, glycosuria, and indicanuria which I am describing in my cases, the inference is unavoidable that large amounts of urinary uric acid are by themselves of slight importance, probably constituting only one of a group of urinary phenomena.

The indican which has been found in these cases has not been constant. Even whilst it has generally been present in cases of digestive disturbance, this has not always been the case. In examining these patients and in looking over the histories, I have always had in mind the series of brilliant and original articles by Herter, and I have endeavored in every way to trace the relationship between the indicanuria and intestinal disturbances. But I have finally come to the conclusion that the indican, like the uric acid, is only part of a general disturbance of

metabolism, and that the decomposition, as represented by indican, of the proteids under the influence of bacterial action cannot be studied alone.

Urobilin is also a substance which has not been constant. At first, I thought that it would bear some relation to anæmia, but this has not been evidenced in my cases, for neither the facial aspect, nor examination of the heart for anæmic murmurs, nor testing the blood by the hæmoglobinometer has demonstrated any lack of corpuscles in some cases where the urobilin was present, although urobilin was found in several in which anæmia was demonstrable. Several cases in which urobilin existed were slightly jaundiced, and the urobilin, therefore, may have been a transformation in the urine of normal bile-coloring matter.

The hyaline casts that have been occasionally found have been a source of considerable solicitude to me, because I am well aware of how often other substances are mistaken for them. Repeated examinations, however, verified in every possible way, have shown me that the observations in this respect are accurate.

From these cases and these examinations, therefore, it would appear that most cases of neurasthenia and many cases of functional nervous disease will show, with precise examinations, slight amounts of albumin, sugar, excess of uric acid, and occasionally hyaline casts, whilst the excretion of urea is about normal. In other words, these cases excrete abnormal amounts of the terminal products of nitrogenized and hydro-carbonaceous metabolism. What becomes of the fats I do not know, unless, as is probable, they pass off through the lungs in the form of carbonic acid. It is probable that these delicate tests show greater amounts of sugar, albumin, uric acid, and hyaline casts in certain conditions than are found in states of health, although I have had many analyses made in which none of them have been found. For these reasons

I have believed that the following conclusions are justifiable:

1. Many functional nervous diseases, especially neurasthenia, can constantly or well-nigh constantly be accompanied by albuminuria, glycosuria, excess of uric acid and oxalate of lime, and occasionally by excess of urea, indican, and hyaline casts.

2. It is probable that these urinary products are results rather than causes of disease.

3. These conditions very probably represent what has been called lithæmia.

4. It is probable that many, if not most, of these cases are not cases of early nephritis.

5. It is possible that there are different albumins in the urine, and that upon the determination of these will rest the future diagnosis of nephritis from other diseases.

The treatment of these cases is not as yet, in my opinion, a satisfactory one. By this, I do not mean to say that most of them cannot be cured, but that our present method of treatment must be prolonged, precise, and of a kind that interferes seriously with the patient's ordinary occupation. Evidently, the increased metabolism that is indicated by these urinary findings must be either the result of nervous derangement, localized derangement of some one organ or set of organs, or some error in the chemical nature of the food supply. The latter theory has been, and is to-day, the popular one in the medical profession. But it has only a very slight basis of fact, as a careful perusal of physiological treatises will show. It is true that we have an idea of the main actions of the digestive fluids, but we are far from having an accurate knowledge of all the chemical transformations taking place in the stomach and duodenum, influenced by the presence of the living tissues surrounding them. It sounds very commonplace to say that the reaction of an isolated digestive fluid in a test-tube is something very different from the capa-

bilities of that same digestive fluid in its proper place and modified by its fellows and all the wondrous mechanism of Nature. If one will read through such careful text-books of physiology as the latest editions of Foster and Arthur Gamgee, this truism will become more apparent. Yet to-day millions of capital are invested in manufacturing firms upon the blind faith that physicians seem to have in the accuracy of their knowledge of digestion and assimilation. In the cases which I have been describing I have not found any advantage from any one system of diet. I have tried the exclusion of nitrogenous material, then the avoidance of the hydrocarbons, then total abstinence from sugar, all without any avail, except that in certain cases a temporary reduction of the proteids has been of very perceptible advantage for the time being. In all the cases, however, in which there was a melancholia or hypochondria, or epilepsy, or vertigo, or neuralgia, I have not found that diet had the slightest influence upon the nervous symptoms. I do not, therefore, believe that the urinary products are due alone to error in the food supply. On the other hand, I have again and again seen cases of neurasthenia and melancholia recover after proper treatment of the nervous symptoms, and then the albuminuria and glycosuria would gradually disappear. I think that I am therefore warranted in concluding that the nervous disease is the cause of the altered metabolism in these cases. For therapeutic purposes I think that all these cases can be divided into three classes: First, cases without mental or nervous symptoms or intestinal disturbances; second, cases with intestinal disturbances, with or without mental or nervous symptoms; third, cases with marked mental or nervous symptoms.

1. The light cases without mental or nervous symptoms or intestinal disturbances are best treated by nitro-muriatic acid, twenty drops in a wine-glass of water three times a day after meals; or in some instances Haig's prescription, of

nitro-muriatic acid before and from two to five grains of salicylate of soda after meals, will answer best. I do not think that larger doses of the salicylate of soda are beneficial; indeed, I seldom go beyond two grains, for more than this is apt to depress the patient. Laxatives should be used gently and carefully in all these cases. Usually I have found Rochelle salt to be sufficient—a drachm in a tumbler of water before breakfast; but it is a great mistake to use the larger doses up to a half-ounce that are often advocated. Sometimes it will be well to use a few small doses of calomel before commencing with the Rochelle salt, say two grains at bedtime for one or two nights, or one-tenth grain given every hour for five hours, and followed next morning by the laxative. Where the constipation is very obstinate I have found that one-tenth grain of aloïn once, twice, or three times daily will be quite effective, and if it acts too precipitately it can be combined with advantage with two grains of the extract of cascara sagrada.

2. The cases in which there is intestinal disturbance may be subdivided into two classes: those in which there is intestinal disturbance alone, and those in which this is accompanied by mental or nervous symptoms. In both, the intestinal disturbance should be mitigated, if it be possible to do so, but in the cases with mental or nervous symptoms, the relief of the intestinal symptoms will not be sufficient, and can very often not be accomplished until the mental or nervous symptoms have been relieved. For the intestinal disturbance I have long given up the use of pepsin, as I believe that it only relieves temporarily, and its continued use will often aggravate. Sometimes pancreatin, in a combination that I shall speak of shortly, will act for a short time very beneficially. My patients have derived much more benefit from the subgallate of bismuth, salol, calomel, subnitrate of bismuth, codeia, laudanum, and salines. Of all these I believe that

the subgallate of bismuth and salol are the most effective, in doses respectively of five and two grains three times a day. It is usually advantageous, however, to preface their use with a few doses of calomel in the manner that I have already described. In certain instances of great discomfort after meals, five or ten grains of the subnitrate of bismuth will act like a charm, but it is not a reliable remedy. Where there is continued pain in digestion that cannot be relieved by these measures, moderate doses of codeia, one-fourth to one-third of a grain, three times a day, will be found to be very beneficial, or if this does not answer, five or ten drops of laudanum may be substituted. These opiates, however, should never be continued very long, in my experience. In all cases, however, care should be taken to have one or two easy movements of the bowels daily, and this can be best attained by the methods already described. Naphthol, beta-naphthol, and guaiacol have not done well in my hands; I have not found them to compare in efficacy with the drugs I have just mentioned.

3. The cases in which there are nervous or mental symptoms with or without prostration, must be treated from an entirely different standpoint. The laxatives and digestives alone will often greatly aggravate the disease, and in this class of cases calomel is an especially dangerous remedy, for I have a number of times seen its use in melancholia followed by a violent outbreak. Rest, or more properly speaking, restriction of the expenditure of energy, is a *sine qua non* in the treatment of these severer cases. The principle of the rest treatment as advocated by Dr. Weir Mitchell is invaluable in these individuals, but the application that he makes of it I do not consider to be always judicious. Absolute and prolonged rest in bed is not necessary in cases of neurasthenia. I have seen many neurasthenics put on their feet after eight to twelve weeks of rigid rest treatment so ruddy and fat that it was

a matter of painful surprise to find them incapable of more than slight exertion for weeks or months. This has led me to seek for modifications of Dr. Mitchell's plan. I have reluctantly abandoned massage; in the first place, because I believe that in many cases of neurasthenia it actually prolongs the weakened condition, whilst I have seen it aggravate cases of melancholia. Even in the severest cases I have found it quite sufficient to put the patients to bed for a few days or a week, then have them up and about in the afternoon, then simply stay in bed until ten in the morning and go to bed by eight or nine at night; whilst in the lighter cases, the interdiction of walking, lying down for a few hours on a lounge each day, and getting long hours of sleep, may be all that is necessary. By these methods, patients never lose the use of their muscles. They simply become somewhat more incapable of exercise, and that is easily rectified when the time comes. In cases of melancholia, it is almost impossible to keep the patient in bed, as the morbid introspection renders the quietude well-nigh unbearable, although, when rest can be obtained, it is always beneficial. Next to the enforced rest, I have found that the ingestion of large, what would often seem to be surplus, quantities of food is most important. This must, however, be done systematically, and day by day the food should be prescribed in increasing amounts until, at the end of ten days or two weeks, three full meals a day and a quart of milk should be taken in the twenty-four hours. In some cases of melancholia the aversion to food is so great that proper alimentation becomes a serious difficulty. When this is the case, I have always found that a threat of feeding by the stomach-tube was all that was needed, inasmuch as patients dread this and will compromise on any terms rather than have it. I have often, however, been forced to actually proceed as if I were about to carry out my threat, but in no single instance have I really had to do it, as the sight

of the tube has been sufficient. Rest and surplus alimentation are usually of basic importance in these cases, but they will not do everything. In melancholia, the careful and systematic use of opium and hypnotics is unavoidable; and in neurasthenics, iron, malt, and cardiac stimulants (the best of which is strychnine) are necessary. Properly (that is, systematically and vigorously) treated, these cases are of excellent prognosis. Left to run their own course, however, they are exceedingly chronic and apt to leave a condition of invalidism behind them.

DISCUSSION.

DR. A. L. LOOMIS.—This interesting paper of Dr. Gray has brought before us some very important points. Ever since albumin was first discovered in the urine, and was regarded as a sign of nephritis, there has been a great diversity of opinion as to the true significance of small quantities of albumin in the urine. From the records which have been presented to us this evening, and perhaps from our own experience as well, we must recognize the fact that small quantities of albumin are met with in a certain proportion of cases of functional nervous disease. When one is asked the true significance of this trace of albumin,—what it really means,—he will be compelled, it seems to me, to answer the question largely from his own personal experience and observation. Many hold the opinion that a small amount of albumin in the urine is of really no significance. There are others who believe that even a large quantity of albumin in the urine, without casts, has no significance as an indication of nephritis.

I am of the opinion that persistent albuminuria, however slight, always indicates an abnormality somewhere. That abnormality may not be found in the kidneys; it may be found, perhaps in the blood or in the variations in the blood pressure, or in faulty metabolism, or in faulty elimination of the materials taken into the body. One can hardly think seriously on this subject without coming to the conclusion that even a very slight quantity of albumin in the urine, if it is persistent, means that there is something seriously wrong in that individual. I do not refer to those

cases of temporary albuminuria which are the result of accidental causes, and which indicate neither disease of the kidney nor of any other organ. I refer to persistent cases of albuminuria.

The records which Dr. Gray has given leave no room for doubt, it seems to me, that in some cases of functional nervous disease, albumin is constantly present in the urine. When the question comes to us, What does this mean? I think it can only be answered that it means that there is something wrong in certain regions of the body. Dr. Gray, in his paper, referred to high arterial tension. If he could have followed those cases throughout the twenty-four hours, he would probably have found that the high arterial tension existed even during their sleeping hours. High arterial tension indicates feebleness, and neurasthenics are always feeble. There is a fault in their blood pressure, and that error will produce albuminuria.

Lithiasis was referred to by Dr. Gray. This is a condition which most of us have been accustomed to regard as one in which there are certain nervous phenomena. It is not necessary, in order to have lithiasis, that a man should pass large amounts of uric acid, but if he has a faulty metabolism, sufficient to produce an extra amount of secretions, he has that condition which we may call what we choose, in which certain nervous phenomena are produced as the result of this increase in the products of excretion. These increased products entering into the circulation form the first step in the production of arterial fibrosis. A man with a slight amount of albumin in his urine, and increased arterial tension, may go on for ten years or more without showing any furthur symptoms; there may be no arterial tension, nor cardiac hypertrophy, nor any of the other advanced signs, but there are variations in the blood pressure in his kidneys sufficient in extent to produce albuminuria. You can predict the future of that man just as surely as you can the rising of to-morrow's sun. His condition will go on to a hypertrophied heart and to hardened arteries, and he will begin to grow old before his time.

In the examination of the urine for albumin, it appears to me that if we go beyond Heller's test, we enter into a refinement which is unnecessary. I have never yet met with a case in which Heller's test was not sufficiently

accurate to show the presence of albumin in the urine if any existed. In testing the urine for albumin, all factors of uncertainty should be eliminated, and if this has been done, and serum albumin is constantly detected, then we may be quite certain that some abnormality exists.

I see more old men now than I formerly did in my practice, and I have yet to examine the urine of a man past the age of sixty years and fail to discover hyaline casts. In men of this age, too, you will usually find increased arterial pressure, and a changed condition of the arteries. If a patient over the age of seventy comes to me, and on examining his urine I find albumin and hyaline casts, I do not think it is worth while to inform him of this. His renal disease will not carry him off. He simply has an old man's kidney. He has started on the downward road of life. His arteries will gradually get harder and harder, and his heart larger, and with these conditions will come on all the infirmities of old age.

DR. A. H. SMITH.—I have had several cases recently, similar to those reported by Dr. Gray, and can follow his description quite readily from my own experience. Within the past two weeks, two gentlemen, both of them having enormous financial interests which they have been watching with intense solicitude during many months, have come to me complaining of symptoms indicating nervous exhaustion. In each case I found, or rather an expert in urinary analysis found, a trace of albumin and sugar, an excess of uric acid, more calcium oxalate than there should be, a little too much indican, and a few hyaline casts. One of these patients presented these symptoms two years ago, and his urine has improved rather than otherwise since then.

It seems to me that in our discussions on this subject of albuminuria, we lose sight of the fact that there is albumin and albumin. Various substances of this nature are produced in the course of the metabolism that occurs in the system. Changes are constantly occurring in the materials that are introduced into the body, and at a certain stage in this process the product is produced which we call albumin. A substance which at one time is albumin may cease to be albumin, or a substance which is not albumin may become albumin. No two specimens of

albumin exactly correspond with each other. A substance is, therefore, called albumin, not because it contains so and so many atoms of oxygen and hydrogen, carbon, nitrogen, and sulphur—for these constituents are variable—but because it corresponds, or will respond, to certain arbitrary tests. Any substance that will do so and so under such and such conditions is called albumin, and yet there is a wide diversity between different specimens. We have a dozen kinds of albumin. Our conception of albumin is at fault so long as we fail to recognize that it is a progressive product. It does not represent a certain definite state in a chemical process, but it is a stage in a continuous process. If we take a normal specimen of urine and agitate it with one third its quantity of sulphuric ether, a gelatinous substance is separated, which may be so dense that it forms a plug at the top of the tube. Here is a specimen in which the plug is so firm that the tube can be turned upside down without spilling the urine at the bottom. That substance is not albumin. It will not respond to the tests that have been set for albumin, but it is an albuminoid. In the process of metabolism it would probably go on and become albumin, or else it has been albumin and is now in one of the retrogressive stages. It probably exists in all specimens of urine. Some of the substances found in the urine by the more delicate tests, which appear to be analogous to albumin, have probably no significance whatever, whereas we know that that substance which is thrown down by heat or nitric acid is a very ugly constituent of the urine. In some cases, I think the small quantity of albumin in the urine is due to irritation from the presence of crystalline substances. I have found it associated with oxalate of lime.

I desire to enter a protest against the inclination which exists to refer everything that takes place in the body to simple chemical laws. There is that in living structures which is absolutely different from anything that occurs in dead matter, and we go astray every time we lose sight of that fact. The chemist may be able to explain the ordinary changes that occur in the body, but suppose a certain person is subject to great mental strain, and as a result abnormal products are formed: shall we say that mental worry is a chemical agent? It is a vital action, and vital energy and chemical energy are entirely different.

Regarding the appearance of small amounts of sugar in the urine, it has seemed to me that its presence there may be explained by the theory that in conditions of extreme mental activity such as we are apt to find in neurasthenics, the cerebral vessels may be acted upon in such a way as to produce an irritation in the diabetic center, which, we know, is situated very close to the vaso-motor center. If we suppose the brain to be continually excited and worried by mental causes, it is not at all impossible that an irritation of the diabetic center will be produced, similar in kind to what we get by inserting a needle into the floor of the fourth ventricle. This may not go on to the development of well-marked diabetes, and it may cease when the cause of the disturbance is removed.

As regards the treatment of this class of cases, I have, in my practice of over thirty years, failed to see much benefit derived from the use of pepsin, pancreatin, or similar agents; we must depend rather upon hygienic regulations and certain restrictions in the diet.

DR. C. A. HERTER.—I have very little to add to this discussion. I have followed the reading of Dr. Gray's paper with the greatest interest, and must state that my results do not correspond with his in many particulars. During the past few years I have made a careful chemical examination of the urine in over two hundred cases, testing for uric acid, urea, sugar, albumin, and indican. Most of these patients were suffering from some form of functional nervous disease, or some form of gastro-intestinal disturbance. Forty of the number were cases of epilepsy, nine melancholia, eight chorea, and a large number of cases of neurasthenia, and in only two of those cases was albumin found in the urine. Sugar was found in only one case. I think it is very important to distinguish and settle upon what we are to call albumin, and what not. I think we should include only serum albumin, although Dr. Gray has not made this distinction. I believe if we do not find serum albumin by means of Heller's cold nitric acid test, or by heat and nitric acid, or by picric acid, its presence should be excluded.

As regards the presence of uric acid in the urine: In the great majority of cases of neurasthenia, I found the amount of uric acid to be increased. I do not regard this

excess as an absolute sign of disease, unless the excess is large and persists for a considerable length of time. A man in robust health may have a temporary increase in his uric acid; this I think is of little significance, but when the urine contains from fifteen to twenty-five per cent. more of uric acid than it should, and this excess continues for some time, it means that there is a great disturbance of metabolism. Dr. Gray, in his paper, did not refer to the observations recently made by a German investigator, to the effect that the increased amount of uric acid is due to the excessive breaking down—or excessive metabolism of a peculiar kind—of the nuclein from the nuclei of the white blood cells. I believe those observations explain, in a general way, the nature of this uric acid excess.

As regards indican in the urine, its presence there certainly cannot be regarded as dependent on disordered metabolism. It has nothing whatever to do with metabolism. It is always due to putrefactive changes of albuminoid material in the intestine. I have brought forward elsewhere evidence in support of this view.

I believe, with Dr. Loomis, that arterial fibrosis depends on the accumulation of waste proteid material in the blood. I believe those cases to which Dr. Gray referred, in which the arterial tension is high, will probably develop kidney disease. I have talked with life insurance examiners on this subject, and they expressed the opinion that those cases in which albumin exists in the urine for a long time usually develop fibroid kidney.

DR. GRAY (closing the discussion).—While I am willing to believe that the persistent occurrence of small amounts of albumin in the urine will, in the course of time, lead to arterial fibrosis and cardiac hypertrophy, yet it has not done so in any of the cases which have been under my careful observation during the past two years. Furthermore, I can point to a number of cases of neurasthenia that have been under my care at various times during the past fifteen years, and although the urine of these patients contained small amounts of albumin, not a single case, to my knowledge, has developed into indubitable renal disease. In some of them, in which I expected to find granular and other casts, the symptoms yielded to

ordinary treatment, and there were no further signs of a serious kidney lesion.

Dr. Herter stated that the results of his experiments differed from mine, and that he regarded the nitric acid test for albumin sufficiently accurate. Again and again have I examined urine with nitric acid and found no traces of albumin, and still there was something suspicious about the specific gravity of the urine, which would lead me to procure another sample, and on examining this with Millard's test, albumin was found. I regard Millard's test as much more sensitive than the nitric acid test. I am somewhat at a loss to understand why Dr. Herter should regard serum albumin as the only albumin; certainly this assumption is largely arbitrary.

Stated Meeting, May 3d, 1894.

A NOMENCLATURE FOR THE DIFFERENT CLASSES OF INFECTIOUS DISEASES.

By W. H. THOMSON, M. D.

Medical terms become important as such in proportion to their influence upon conduct. When the effect of the wrong use of some such terms may needlessly disturb the affairs of a household and seriously alter the natural relations of its members to each other, or alarm an entire neighborhood, or empty a town, or greatly derange the commerce of a country, it certainly behooves the medical profession to endeavor to prevent all such misunderstandings by making the meaning of such terms as definite as possible. Instead, however, of this having been the case in the past, there is no more unpleasant feature in the history of medicine than the long confusion, often marked by angry controversy, which has characterized the use by medical men of the terms intended to be descriptive of the nature of infectious diseases.

How untoward in its results such confusion may be is well illustrated by the variety of ways in which the term "contagious" has exerted an influence for evil both on the profession and on the public. No word can escape entirely from association with its primary meaning, and particularly no descriptive name can wholly lose the original hue of its derivation. To spread by contagion means to spread by touch, and it is impossible thoroughly to dispossess anyone's mind, whether medical or non-medical, of certain suggestions connected with this first significance of the term. It was thus that this primary meaning of the word

contagious generated the notion that a sort of continuity, like the spread of a fire among combustibles, was the essential element in all contagious diseases, and hence if any other diseases did not so spread, but could spring up separately without contact with previous cases, it was accordingly inferred that there could be no community between them and the contagious diseases. The term contagion, in fact, monopolized the mental conception entirely, to the exclusion of every other, with the result that when a particular epidemic disease was shown to spread in a community without personal contact with the sick, it was forthwith pronounced to be wholly different in kind from contagious diseases, and the public was exhorted to dismiss all fear of its having any relationship whatever to such diseases, and to act accordingly.

Asiatic cholera, for instance, was easily shown to be a non-contagious disease, because in every epidemic of it, especially at the beginning, persons fell victims to it who were known not to have been near any one sick with cholera, while those who themselves attended the sick, living, for example, on the banks of a stream, were not as often attacked as persons who lived at a distance down that stream. The next logical conclusion in this train of reasoning was that cholera had nothing to do with human intercourse any more than ague has to do with human intercourse. This seems to have been the opinion of the Medical Society of Massachusetts, when, in the year 1855, it promulgated with much emphasis, for the benefit of the public, the doctrine that Asiatic cholera was in no sense contagious, but rather that it was a miasmatic disease, dependent upon some general element suspended in the atmosphere.

Ere long, however, the close and unmistakable relationship of the spread of cholera to the routes of commerce or of travel obliged many to admit that this disease had something to do with human intercourse. But if so, such a fact

was conclusive against its being a miasmatic disease, for miasmatic diseases never leave their localities or go on journeys. Hence, as cholera was neither contagious nor miasmatic, it was proposed to call it "infectious." But this designation was equally unfortunate with contagious as a specifically descriptive term, though in an opposite fashion, for whereas "contagious" strictly limits the conception of the mode of extension, "infectious" defines nothing as to mode, for according to its original meaning it can be applied equally well to contagious, to purely miasmatic diseases, and also to the class which is neither miasmatic nor contagious, like typhoid fever and cholera. A man can be as truly infected with malaria by going to a malarious locality as he can become infected with small pox by entering a room where a small-pox patient lies; and again, he can be likewise infected with typhoid fever and with cholera by going to a town where these diseases are epidemic. Therefore, to term cholera infectious only served to make the conception of its true nature more vague than ever.

Vague fear is the mother of panic, and vague terms are the most potent generators of vague fears. When medical men themselves are not sure how the different classes of epidemic diseases should be distinguished from each other by descriptive names, the public can scarcely know what to think about them, and hence is open to the occurrence of as needless a fright as that about cholera which cost the city of New York so much in the autumn of 1892.

This subject, morever, has assumed of late quite a new phase and importance by the fact that the progress of medical knowledge has led us to add to the list of specific infectious diseases both tuberculosis and ague. But if the profession is to announce that pulmonary consumption is truly infectious, the public has every right to ask just what we mean by it. Is infectious the same thing as contagious; and if not contagious, then what is it? We cannot mini-

mize the importance of this matter, for without more specific explanation it will be impossible to prevent painful results in conduct toward consumptives, owing to the natural interpretation by ordinary persons of the term infectious as implying necessarily some form of contagion in this very general disease.

Moreover, it is to the immediate interest of the profession, as well as of the public, that the differences in the modes of infection between the various classes of infectious diseases should be clearly defined, so as to prevent both disbelief in, and opposition to, our recommendations as a profession about them. One of the chief obstacles to the diminution of tuberculosis, either by precautions taken with consumptive patients or by the sacrifice of tuberculous cattle, will be the denial that consumption is an actively infectious disease, simply because common experience shows that it is by no means contagious. A general incredulity is already manifest in newspaper comments on this subject, which is based upon appeals to the infrequency with which consumptive husbands or wives communicate the disease to their life partners. We even see communications signed by physicians taking the same ground on the same argument, with appeal to the Report of the Collective Investigation Committee of the British Medical Association of 1883, on this point, as disproving the communicability of consumption at all, and hence they inveigh against the inhumanity of the doctrine of the contagiousness of phthisis, thus plainly supposing that the terms infectious and contagious are interchangeable.

Meantime it cannot be said that, on account of the insufficiency of our knowledge, we as a profession are still unable so to distinguish the chief characteristics of infective diseases that we cannot classify them accordingly. On the contrary, in no respect has our advance of late years been so distinct and gratifying as in the discovery of just how infective diseases are infectious, and often to arrest their

spread by specific knowledge on that very point. There has not been, for example, in all history such a striking illustration of the supreme advantage of definite scientific acquaintance with the mode of infection as in the case of the recent arrest, in the very act, so to speak, of Asiatic cholera at the great assemblage of pilgrims in its native habitat at the Kumbh Fair at Hardwâr in India. As a conclusive illustration of our contention we beg leave to cite the following extract about this case from a lecture delivered, February 14, 1894, by Dr. E. Klein, F. R. S., before the London Institution.

Dr. Klein says: "A well-known fact, which has received unfortunately a great many illustrations, is this, that pilgrims in India carry the contagium of cholera from the fairs or festivals to which the disease is brought from the endemic area to localities which were previously free from cholera." We may in passing note that even Dr. Klein speaks of the contagium of cholera, whereas cholera is in no proper sense a contagious disease. Need we wonder, then, that with medical authorities using such loose terms about infectious diseases ordinary people will dread the contagiousness of cholera, tuberculosis, and typhoid fever? "One such fair," he continues, "is particularly notorious, and it has in the past always been a source of the greatest anxiety to the government of India. This is the great religious festival, or Kumbh fair of Hardwâr, a town on the Ganges, but situated outside the endemic area of cholera. This great Kumbh occurs once in twelve years, and it is attended by large numbers of pilgrims, a proportion of those coming from districts in which cholera is always endemic. It has thus frequently happened that this great concourse of pilgrims has been followed by a wide diffusion of the disease. The great Kumbh is principally a religious festival, and is looked upon by Hindoos as one of peculiar sanctity, and the very aim and object of the pilgrimage is to bathe in the sacred

Ganges and to drink its holy waters. In 1891, when the last Kumbh fair was held, 800,000 to 1,000,000 pilgrims assembled at Hardwâr, and to get an approximate estimate of the enormous pollution to which the sacred Ganges is subjected on this occasion, and the great risk from cholera to which those who drink of its waters are exposed, I will mention that Dr. Simpson, the able health officer of Calcutta, in describing the scene at the 'sacred pool' at Hardwâr, somewhat retired from the rest of the river, to bathe in which and to drink whose waters the pilgrims gather in such multitudes states: 'As the bathing went on the clear stream became a muddy one; that from April 8th to April 12th there was always passing through the sacred waters 'a seething mass of humanity' in constant motion through the pool at the rate of four hundred to five hundred per minute. You can easily picture to yourselves that a few cases of cholera introduced into such a multitude would easily cause not only an outbreak of cholera there and then, but would, by the returning pilgrims, be carried far and wide. Thus a sanitary commissioner says of the Kumbh on previous occasions that very little remains on record, but that little is a record of disease and death. So grave was the outlook that the question of prohibiting the fair in April, 1891, was seriously discussed, and the official opinion of many government officials of great experience was that the most complete sanitary arrangements would be powerless to prevent the spread of cholera should the fair at Hardwâr be allowed to take place."

But in 1891, armed with the knowledge of just how cholera infects, and that without its being taken in food or drink cholera cannot travel six inches, the British sanitary authorities grappled with the problem by allowing the fair to be held, and then stamped out cholera every time it appeared in the midst of this Asiatic crowd, as effectually as a fire can be easily extinguished if taken just at its beginning.

Nor, with respect to other infective diseases besides cholera, is our knowledge less definite as to the ordinary modes by which they are propagated. We have now got rid utterly of that nightmare of a miasm, or deadly diffusible gas, which weighed upon the minds of medical men for so many generations whenever they thought of epidemic diseases, and enough is known for us now to tell the world both what infection means, and what the chief modes of infective propagation are, and hence how to guard against them.

How readily this can be done I beg leave to illustrate by submitting the following classification of infectious diseases for the consideration of the Academy, with the hope that it will serve to invite discussion on a subject of great public importance, namely, the adoption by the profession of a definite nomenclature descriptive of the various classes of infectious diseases.

This classification is as follows:

1. All infectious diseases are due to the presence of their specific living micro-organisms in the body.

2. Infectious diseases are divisible into three classes—(1) the communicable; (2) the non-communicable; and (3) the septic.

3. The communicable diseases are those whose origin is from an animal body. Examples of which are small pox, Asiatic cholera, and tuberculosis.

4. The non-communicable infectious diseases are those whose origin is not from an animal body, but from a place or thing. Examples of which are ague, yellow fever, and miasmatic diseases in general.

5. The communicable diseases are divisible into two classes, according to the ordinary modes of their communication:—(1) into the contagious, and (2) into the non-contagious communicable diseases. The contagious communicable diseases are those in which simple proximity to those infected with them is sufficient to communicate the

infection to those susceptible to them. Examples of which are typhus fever, scarlet fever, small pox, measles, diphtheria, mumps, whooping-cough, etc. Isolation of those sick with them, therefore, is needful to prevent infection.

The non-contagious communicable diseases are those in which the communication is not by simple proximity to the sick, but through intermediate means of communication. Isolation of those sick with them, therefore, is neither needful nor effective, in comparison with measures directed against the intermediate means of the infection. Examples of these diseases are typhoid fever, Asiatic cholera, and tuberculosis.

6. The septic infectious diseases are those in which the infection has gained entrance through a wound, or damage, by inflammation or otherwise, of the skin or of a mucous membrane. Examples of these are surgical infection of wounds, erysipelas, hydrophobia, tetanus, etc.

Lastly, all infectious diseases can be prevented by measures of disinfection applicable to each.

This statement of infectious diseases and of their respective classes, if correct, would have the advantage, in my opinion, of being intelligible to the most ordinary readers on account of its being free from all technical terms. In the first place, it is desirable, as it seems to me, that a clear conception of the ultimate cause of all infection should be generally entertained. When all infection is understood to be due to living micro-organisms as its primary and essential element, all vague ideas of a relationship to poisonous chemical, physical, or atmospheric instrumentality will give place to more correct conceptions not only of what infection is, but also of what disinfection implies. The latter, indeed, is practically the most important of the two, for it is a familiar fact that people never do things so well as when they know why they should do them. The more thoroughly an enemy is known, the more

intelligently he is opposed, and when that enemy is known to be a living, though invisible, thing, what will destroy its life is seen to be more useful than to endeavor to counteract its doings. Another reason for defining infectious diseases as always due in the first instance to micro-organisms, is that then it will be readily appreciated how their modes of propagation may vary widely, according to the life-history of each species, rather than by only the one mode of contagion. Thus miasmatic diseases will easily be seen to be totally non-communicable as well as non-contagious, because yellow fever, for example, is not derived from an animal source, and only diseases from an animal source can be communicable to other animals. Again, with this conception of the nature of infection, it will not be difficult to demonstrate that some communicable diseases may be able to pass directly from one animal body to another, while others only indirectly, for so do seeds in all nature, even in the highest vegetable forms, present us with precisely analogous contrasts in the modes of their propagation. It has been asserted, for example, that but for the humble ground squirrel, hickory, oak, and chestnut trees would not be able to hold their own in our forests against the trees whose seeds can be carried by the wind.

The term communicable seems to me particularly advisable to designate every disease whose origin as a disease is from some animal body previously infected with it, because this term will then fasten in the mind the fact that no case of such disease has any other cause than by such communication. This conviction, of course, does go with the word contagious, but it at once becomes very indefinite in all infectious affections where the term contagious is inapplicable. Hence some designation is particularly needful which will always mean danger to others, even if there be no evidence of contagion, and the word communicable, as we have defined it, does just that. Let

a disease be known as communicable, whether contagious or not, and attention is at once awakened to the need of preventing such communication. Nothing, indeed, would so powerfully enlist general co-operation with us toward abating tuberculosis, for example, as to have every consumptive patient and his friends become assured that this distressing malady came to him, not *from* earth, air, or water (though perhaps by all three), but came instead to him originally from another consumptive. But I do not know of any other term than communicable which will serve this purpose, for all other terms which have been employed imply more or less the idea of contagion. Now we should not ignore the instinctive recoil of most persons against the word contagion. In an acute disease like scarlet fever, its dread presence is borne as well as may be on account of the hope that ere long it will be past. But for a chronic affection like phthisis, acting like the slow blight of a withering curse, to have it also thought of as contagious is so repellant to patients and to friends alike that every objection to the belief in such a doctrine will be sure to have its effect upon the mind. But with the designation communicable, but not contagious, the loving ministry of friends need in no way be checked by dread of touching the stricken one, while at the same time the name gives an effective note of warning against its further repetition in other persons.

It is with communicable but non-contagious diseases, in fact, that a definite designating term is at present most needed to prevent ignorant fears on the one hand or ignorant carelessness on the other. It was a serious discredit to our profession, as well as to our civilization, that a passenger steamer was dealt with as the *Normannia* was in 1892, when, notwithstanding the great object-lesson which it gave that cholera is not a contagious disease, because not a single case of cholera occurred among its cabin passengers, though they were virtually nearer to it in an

epidemic form among the steerage passengers than if they were all in a city tenement, yet no ship-load of small-pox patients could have been more summarily dealt with than they were. Meantime the absurdity of the usual methods of quarantine service in such a case was perfectly well known by us physicians. A single individual from that steerage crowd, with a slight choleric diarrhœa, might have passed up the Harlem railroad as a laborer, and there infected the water-supply of this city beyond the power of all the quarantine officials in the country to have remedied the mischief. But when confronting the New York State Senate Committee on Cities as a delegate from this Academy to lay these facts before that remarkable body of legislators, I found a state of mind and of intelligence in them on all questions about infectious diseases which one might expect from an equal number of Mohammedan muftis, and no more. In fact, the chairman of the committee, in reply, enunciated the old orthodox Moslem doctrine that God was himself responsible for whatever should happen on the Croton watershed, as he sent the rain which washed things into the river.

Again, what a commentary upon our contemporary medical knowledge is the following instance related to me only last week: A widely-known and highly-esteemed lady, resident in Princeton, N. J., died this past summer in Chicago, from typhoid fever. Arrangements for the funeral had all been made at her home, and prominent persons were chosen as pall-bearers, when the undertaker showed the husband a certificate that according to the laws of New Jersey public funerals were prohibited in the case of persons who had died of typhoid fever. In the case of tuberculosis, the need of such a designating term as communicable is still more urgent as a warning against its most insidious ways of gaining entrance. Thus a consumptive might not communicate the disease to any one in his home, and yet so thoroughly infect his down-town

office by constantly expectorating on its floor, that he would communicate the disease to any one who swept that floor, or even to another person who hired the same office after him.

Meantime, I would restrict the appellation "contagious" to those diseases which are so directly communicable from body to body that mere proximity to the infected is ordinarily sufficient to occasion the disease in others. That these diseases should have a term which would adequately suggest this element in them is very desirable, because there should be no doubt in their case of the immediate necessity for isolation. This question of isolation, indeed, overrides all others when it is raised, for it always means inconvenience and hardship, at least, if not cause for widespread alarm. I can see no objection to the old term contagious, if we use it in the sense ordinarily understood by the public, namely, a disease which may be contracted by going near one sick with it, without implying anything else about its mode of communication. In this sense the term has become as settled in our language as any other English word, and on that account never can be displaced. It will always be used whether we medical men wish to retain it or not, while new words, however good, have to wait a long time ere it be known whether they will be allowed to become naturalized in a language or not. Moreover, the precise mode of communication has nothing to do with this term, or, for that matter, with any other designation of infectious diseases, for with the majority of these affections we are still ignorant of the precise mode of their infection, and hence we should have to wait indefinitely if we would postpone classifying them until we could define them according to their specific ways of entering the body. What we do know about those diseases which seem to pass directly from body to body is that whether they pass in one way or another, they do so commonly only at short distances, and that fact is enough to justify the choice of a

term like contagious, which sufficiently expresses the danger of approaching the infected, without necessarily implying anything else, while it also distinguishes them from the communicable but non-contagious diseases, in which the danger is as great, if not greater, at a distance than in the immediate neighborhood of the sick.

I have chosen the term septic to designate the third class of infectious diseases, mainly because of the familiarity of the public with the word as applied to what may be termed infections by inoculation. Antiseptic surgery has made the idea and the conditions of sepsis both widely and correctly known, and to my mind it is no objection to the term that its use in medicine is rather restricted to purulent infections, for tetanus and hydrophobia are as strictly inoculable diseases as pyæmia.

In this classification it is carefully stated that it is based upon the ordinary methods of propagation among the respective classes. That the active agents of some communicable contagious diseases may be carried to a distance by various media, as scarlet fever has been asserted to have been conveyed by letter, or in milk, does not militate against the statement that the usual mode of communication is by proximity to the sick. All infectious diseases probably can be communicated by inoculation into the blood, as it is claimed that ague has been so transmitted, but such wholly exceptional methods no more invalidate the designation of these diseases according to the normal modes of their increase than to object to certain plants being termed tropical because they are often found growing in greenhouses.

Finally, it seems fitting to direct attention to the fact that all classes of infectious diseases are alike capable of being disinfected out of existence, because the public should be educated to the possibility of ridding the earth of the great cause of unhealthy places or climates, namely, miasmatic diseases. It is doubtful if an unhealthy climate

as such exists on the globe, except in the Arctic regions from their extreme cold. Rather, it is not too much to hope that in the coming century the world will become so much wiser that a proper proportion of the enormous sums now wasted upon armies and navies will be transferred to sanitary bodies instead, with the result, already seen to be feasible, of disinfecting the soil by drainage and by other well-known means, until malarial fevers and affections will be matters only of history.

I have taken the liberty of bringing this subject before the Academy because it seems to me to be well within its province to take steps toward securing a uniformity in the nomenclature of the different classes of infectious diseases, such as would be justified by the present state of our knowledge on the subject, and which would greatly tend to the advantage both of the profession and of the public.

DISCUSSION.

DR. T. M. PRUDDEN.—I think that any attentive reader of the medical literature of to-day must be fully convinced of the importance of some sort of reform in the nomenclature of the different classes of infectious disease, and that the Academy should feel its indebtedness to Dr. Thomson for laying the matter so clearly before it. The subject is beset with peculiar difficulties. If it were the Academy alone, or the physicians of New York city alone, who were interested in this question, it would be a comparatively easy matter to classify these diseases and arrive at a mutual understanding as to the meaning of certain terms. The fact is, such terms as contagious and infectious, while having a definite enough etymological meaning, have been used in a variety of senses. The original meanings of these words were not very clear, and to-day we must get new meanings for them if we would retain them at all. It might be possible at a general congress of medical men to have new meanings accepted for these words. If our knowledge of the causation of contagious diseases were complete, it would simplify this subject a great deal. As it is, we keep on putting new

wine into old bottles, and the wine is fermenting and the bottles are pretty badly demoralized. We can either abolish the old words contagious, infectious, and miasmatic altogether, or we can invest them with new meanings, or we can get new words. My own opinion is that, starting with a precise conception of what is meant by the term infectious diseases, such as has been indicated by the speaker, we should adopt a rather simple classification. For instance, we might safely divide all these diseases into two great classes, the communicable and the non-communicable; this would give us a very solid basis to start on. Whether we could go far beyond that or not at the present time I think would be a question for serious consideration.

DR. J. W. BRANNAN.—I think Dr. Thomson has brought up a very important subject for consideration, and if the Academy could do anything to put it on a good basis it would be well worth the attempt. I agree with Dr. Prudden that there are many difficulties in the way. I know that Dr. Thomson has had it under consideration for many years. If we can only secure a clearer idea of the exact meaning of the words contagious and infectious we shall certainly have accomplished a great deal.

I agree entirely with Dr. Thomson in calling all these diseases infectious. I believe in dividing them into the communicable and the non-communicable, but I should not attach the same meaning to the word communicable that he does. He states that a communicable disease is one that is communicated from one animal body to another. I should say that a communicable disease is one that can be communicated from the sick to the well by any means whatsoever. Yellow fever, for instance, I should regard in one sense as a communicable disease, as the clothing of the patient serves to transmit the disease.

The term "contagious diseases" I should restrict to its old signification, meaning thereby any disease that can be communicated by simple diffusion through the air about the patient. This would include small pox, typhus fever, mumps, etc. Dr. Thomson mentioned, for example, that diphtheria is a contagious disease. That proposition would at once give rise to differences of opinion. There are many men who believe that while diphtheria is a com-

municable disease, like typhoid fever or cholera, it is not contagious: that it should not be classed with scarlet fever, mumps, etc., which are undoubtedly communicated by diffusion through the air. If we are to secure any general agreement of opinion we shall have to limit ourselves, I think, to a very simple classification.

DR. J. W. ROOSEVELT.—It seems to me that we are getting slightly mixed about words. There is a very practical side to this question, and another which is theoretical. It does not seem to me to be very important whether diphtheria is a directly contagious disease, in one sense, or not. The fact remains that one case of diphtheria will produce other cases in the neighborhood, which makes it quite as contagious as scarlet fever, or any of the other well-recognized contagious diseases. Whether the contagion passes directly from one child to another that is sleeping in the same bed, or comes in through the door or window, makes very little practical difference; the fact remains that if one case of diphtheria occurs in a certain house, other occupants of the house are likely to take the disease.

I understand that Dr. Thomson's intention was that we are to deal with a nomenclature for different classes of infectious diseases that is to be adopted for practical, public use; that we are not to split hairs on the finer details of this nomenclature. It is very important that we should employ terms in this connection that the public in general can understand, or rather, that they cannot fail to understand. If we can adopt a classification of practical value to the public, which shall convey certain definite ideas, I think we shall have done as much as we can, and it is very important that we should attempt to agree upon such a classification.

At the very beginning of this subject we get into trouble about the meaning of the word infection. There is a convenience and advantage in the use of this word as defining a certain class of diseases which, as far as we know, are of microbic origin. This at once gives them a distinguishing feature. Then there are certain diseases whose existence in individuals is a source of danger to other individuals. They can be communicated either directly or more or less indirectly; practically, they are

communicable from one to another. Such diseases should be so classified that their mere titles will carry with them the idea of danger in the immediate vicinity of such patients. The word contagious, I think, is perfectly proper to adopt for such diseases. Communicable may very properly be used to indicate that a disease is capable of passing directly or indirectly from one person to another. This term includes the contagious group, of course. There would be no objection to limiting the meaning of the latter word so that it be only applied to diseases which are, like the exanthemata, directly communicable, and to those which, like phthisis, typhoid fever, or cholera, are harmless if certain known infection roads are blocked.

If we can make the public regard consumption in the light of a communicable disease, and yet have it clearly separated from contagious diseases, like scarlet fever, we shall have done a good deal. If we can only lay aside the notion that we must adopt only such an accurate classification as never yet has been made, and if also we bear in mind the great practical value of Dr. Thomson's suggestions, it seems to me that the Academy would unhesitatingly recommend the use of the classification which he recommends.

DR. S. T. ARMSTRONG.—I think the classification of infectious diseases that Professor Thomson has suggested is one of the most comprehensive I have ever heard, and it appears to me to more nearly meet the indications than any other I know of. As Dr. Prudden has said, it is very desirable that some such classification be adopted, and if the Academy takes the initiative in this matter, it will be prominently brought before the profession, and thus a unanimity of opinion may be secured.

DR. A. HADDEN.—Dr. Thomson's classification of infectious diseases is, as I understand it, intended for public use. The principal thing which the public wish to know is whether a disease is "taking" or not. It is, perhaps, necessary that the public should know whether a disease spreads directly—that is, by immediate contact with the patient—or indirectly, and this knowledge is, I think, most easily taught in the schools. Another thing the public wish to know is, How is a disease "taking"? Is it by direct contact, or through the medium of the air,

or water, etc.? A more minute classification of infectious diseases than is required to protect themselves will not be appreciated by the public at large; their attention is fixed on other things, such as making a living, or delving into other sciences. A point in connection with this subject, which Dr. Thomson did not refer to, is that of heredity—whether a disease is communicable by inheritance or not. It is possible that, in the light of the advanced knowledge of germs in disease, a disposition may exist at present to eliminate it as a cause; but I take it that neither the enlightened public nor the physicians, as a whole, are in readiness to agree to this.

DR. THOMSON (closing the discussion).—I first wish to remark that infection belongs to the phenomena of the living kingdom, and when we say that, we must admit that a perfect classification of infectious diseases is at present impossible. A perfect classification is not possible in botany or in zoölogy. The question is, whether we should not make a classification of infectious diseases which is abreast of our knowledge at the present time. That such a classification should be made, seems to me to be of great practical interest. The commerce of our country, and of New York city in particular, was most unnecessarily disturbed during the recent cholera scare by mistaking the meaning of a term regarding an infectious disease. Tuberculosis, which is an infectious disease, can be disinfected out of existence if the proper means are adopted; the knowledge of how this can be done may be imparted to the public by adopting proper terms. We must give them names which mean something definite. It is on this account that I have offered this classification, beginning with a definition of what infectious diseases are; that they are all due to the presence in the body of their specific micro-organism. Second, that we can divide all infectious diseases into two classes, the communicable and the non-communicable; and, third, that we have the inoculable or septic.

Dr. Brannan seems to think that by the exclusion of yellow fever from the list of communicable diseases, I have laid the classification open to criticism. By communicable diseases, I mean those that pass from one animal body to another. Yellow fever does not spread in

that way. In the numerous yellow fever epidemics we have had, the health officer of the port, whose duty it was to board the infected vessel, almost invariably took the disease, while the physicians and nurses in the quarantine hospitals always escaped, although they were constantly handling the yellow fever patients. The reason they escaped was that they did not visit the vessel or spot which was infected with yellow fever. What Dr. Brannan means is that yellow fever is a portable disease—not a communicable one. By a communicable disease, I mean one that is communicable from one animal body to another. Yellow fever is not, any more than is intoxication by alcohol; the intoxication is communicated through the medium of a wine-glass, not from person to person.

The important question is, How are we to divide the communicable diseases? In my lectures at the college, I have for years divided them into the contagious and the non-contagious communicable diseases. I was rather surprised to hear Dr. Brannan say that there are physicians who deny the contagious nature of diphtheria. I consider it as contagious as a fire spreading to the nearest combustibles.

It is not necessary to dwell long on contagious diseases. Their contagious nature proves itself. The important thing is to adopt a classification that is practical, and that will be understood by the public. These terms must clearly imply that certain diseases spread from one animal body to another directly, while others spread through some intermediate medium.

Stated Meeting, May 17th, 1894.

THE NATURE AND MANAGEMENT OF FUNCTIONAL GASTRIC DISORDERS.

By CHARLES G. STOCKTON, M. D.,

BUFFALO, N. Y.

It is said that things are known when understood, and are understood when interpreted. Concerning the familiar symptom-complex embraced under the term functional gastric disturbances there is much lack of agreement in interpretation, and therefore it is concluded that there exists disagreement as to the nature of the pathology in question.

A recent American writer has arrayed these symptoms to explain them as the result of subacute gastritis. One continental observer attributes the phenomena to an oncoming gastrectasis, and another easily divides the group into a series of special stomach diseases in which hyperchlorhydria stands forth prominently. Ewald, Rosenheim, and Boas separate the disturbances into irritative forms and depressive forms, and then assign to each abnormalities in motion, secretion, or sensation.

Unquestionably there is functional disturbance of the organ in subacute gastritis, and the same holds true of beginning gastrectasis; also there are types that are sufficiently distinct to admit of their description as varieties, and an analysis of these varieties, after the fashion of Ewald and others, leads to a clearer understanding of the nature of gastric neuroses; and yet it would appear as though the subject were open to still another interpretation, of which, in part, it is the purpose of this brief paper to speak.

In the first place, let it be said that the functional gastric disorder is very rarely a primary trouble. In reviewing the records of a series, including between five and six hundred cases of supposed stomach disease, in which at least one complete examination of the stomach contents was made, in not one instance can it be said that functional trouble indubitably arose from a disturbance that was primarily gastric.

This is not impeaching the possibility of such occurrence. It can readily be seen how overtaxing the stomach alone might eventually produce exhaustion and a consequent seated neurosis; indeed, as a contributing factor, perhaps nothing is more important than gluttony, but in point of fact one rarely finds gluttony the essential cause of the neurosis. Something has gone before, or something accompanies the pernicious habit of overindulgence, and as a joint result we discover the neurosis. It is remarkable that the stomach will tolerate patiently so many insults of a dietetic character, both in food and in drink. True, a subacute gastritis may result, but under a diminished appetite it soon subsides, and no functional disorder follows.

Of course this statement may be met by quoting the freely-admitted fact that it is the every-day experience with practitioners to have patients complain of persistent gastric symptoms resulting from injudicious eating. Quite true; a failing gastric function is usually first made manifest by the overtaxing of that function; but even if overtaxed, that function rarely, if ever, remains weak and perverted from the effect of the indiscretion alone. On the contrary, a healthy organism, after a short period of reaction—congestion, if you please, of subacute gastritis—resumes its customary pace. (Note Beumont's oft-mentioned observations on St. Martin.)

What, then, are the influences that invite the appearance of the persistent functional disorders of the stomach?

The question is one that should arise in the practitioner's mind with every case that is presented. This paper is intended as a protest that the question is not oftener raised.

In ratio to one's ability to hunt down and remove the causal factor will be his success in relieving the neurosis—the failure in which has come to be an opprobrium. The stomach, like a mirror, reflects pretty nearly every influence to which the organism is subjected. Irritation to the brain, the cord, the testicle, the uterus, the kidney, or the liver, results in anorexia, or perhaps vomiting. Grief, joy, worry, or pain lead to similar results. When there is toxæmia from constipation, renal or cutaneous insufficiency, or the infections, it is the stomach that raises the alarm. In the cerebral anæmia of naupathia, or of a fainting turn, and in the cerebral congestion of sunstroke, the gastric are in the tumultuous front of the anarchistic functions.

One need not go far for explanation of these phenomena. Perhaps no portion of the organism has so complex a sympathetic nerve supply as the stomach. Robert Ewald's extraordinary exposition of this truth, in his brother's masterly book on stomach diseases, is entitled to mention on all occasions. Just why nature has provided this curious arrangement, it would be interesting, but at this time extraneous, to discuss. Enough to say that it exists. Now, in those suffering from these various irritations, any of which, either through toxæmia or reflex disturbances, may lead to gastric symptoms, if the stomach contents were examined there would be found no evidence of subacute gastritis, none of the "ropy mucus" that inexperienced men so much talk and write about, and which those who have largely studied gastric contents usually fail to discover; but there would be found testimony that there had occurred a serious derangement in gastric innervation, exhibiting itself to some extent as excitement, to some extent as depression, and in some instances it is impracticable to say that either of these states predominates.

An attempt at the segregation or the classification of these gastric phenomena appears to the writer as usually inexpedient, if not unsuccessful. They are mixed symptoms resulting from remote disturbances. Undoubtedly, a stomach having such faulty and spasmodic innervation would now resent bad diet or perhaps any other kind of diet, and yet the trouble is not one, primarily, of the stomach. It is not that the face of the mirror is irregular, but it is because of the distorted figures reflected from it that the picture is hideous.

If these statements are facts, and if they be applied as guides in traversing the maze of the functional disorders of the stomach, it would seem as though they would help us to rightly understand the real nature of those affections.

Let any case of undoubted functional gastric disease be put under examination. There may be eructations of gas or sour fluid (that means insufficiency of the cardia, as a rule); there may be an excess of free HCL (that means a faulty inhibition of the peptic glands, probably); there may be uneasiness amounting to gastralgia (that means undue irritation or hyperæsthesia, one or both). Now, instead of resting content with this diagnosis, admirable and advanced though it may be, and making for himself limitation in therapeutics that are directed toward immediate relief and really nothing else, no matter how successfully they are applied, the physician is asked to look further, having before him the conviction that there is somewhere operating an unthought-of cause that has diverted nervous energy from its ordinary courses and is the present malefactor.

Where will the trouble be found? In the search, it is convenient to examine for the possible presence of, *first*, some centric or eccentric nerve irritation acting reflexly; and, *second*, some toxæmia. The nerve irritation may prove to be some central structural disease or persistent circulatory disturbance; or it may follow excessive mental

strain, as seen in actors, school teachers, and those suffering constant interruptions; or it may be and, in fact, often is accounted for by defects in the organs of special sense, particularly the eyes.

One cannot be unmindful of the doubts that this last statement may arouse in the minds of listeners who have given the question of eye strain large attention, for it is well known that observers differ widely as to the importance of ocular defects in the development of functional disorders. The writer has no contention to make, but a faithful record of personal experience to report.

To leave for the present the matter of the brain and its nerves, it will be found that irritation of the cord, either directly, as from pressure in lordosis, scoliosis, and other sources; or interference with innervation, as in tabes or focal myelitis; or indirectly, as in irritation from hysterical spine, is a common source of gastric disorder.

Further than this the mischief may be lurking in the genito-urinary apparatus. Not alone the uterus and ovaries, but the urethra, the meatus, and the prepuce should be critically examined. A frequent and generally overlooked cause of the disturbance exists in ureteritis, to which attention has been especially directed by Dr. Howard Kelly and by Dr. Matthew D. Mann. Wandering and movable kidney, and renal irritation from hyperlithuria belong to the list. It is not intended to catalogue all the known reflex causes of functional disturbance, since the subject is too trite and perhaps the array is already redundant.

There now remains to be considered the influence of toxæmia in the production of stomach disturbances. One of the most common and important forms results from defects of the kidneys, either dependent upon nephritis or upon functional inadequacy of the organs, a subject fully discussed by Sir Andrew Clark, Casper, A. Peyer, and others abroad, and by Rochester, and Allen Jones in this

country. So alcohol, tobacco, coffee, and other substances may induce digestive disturbance indirectly through the blood and nervous system. Malaria, gout, and tuberculosis often offend in the same manner.

Not to be prolix, there is another potent and, it is believed, hitherto undescribed cause of functional gastric disorder, doubtless operating through the medium of the blood. Reference is had to the very late phases of syphilis. The patients usually give a history of an ancient infection with presumably the usual routine of medication. In several, there is the history of healthy children. As a rule, there is the absence of the ordinary tertiary lesions, although the skin is usually muddy, and in some there exists a moderate anæmia. In the series above mentioned, representing over five hundred cases in which the stomach was minutely studied, there were twelve whose gastric symptoms depended upon late syphilitic toxæmia. Only one of these, a gentleman residing in this city, improved upon ordinary treatment. Fully one half failed to improve after such treatment was faithfully tried, but all of them made immediate, prompt, and lasting improvement when given ascending doses of potassic iodide, and, with one exception, all have been discharged as cured on no other than syphilitic treatment, the other measures having been abandoned. It is interesting to note that in these cases there occurred but little disturbance in secretion. The complaint was of sensory and motor symptoms,—gas, eructations, nausea, distress after eating, and frequently more or less gastralgia. This account has been somewhat amplified because the conclusions are believed to be new.

In reviewing the multitudinous causes of functional gastric disorders, it would be interesting to analyze, with a view of determining whether or not given irritations, particular reflexes, and special forms of toxæmia produce uniform and distinct varieties of gastric neuroses. So far as this matter has been studied, the answer must be in the

main, no. However, there is ground for believing that in a few instances, with a pronounced given reflex irritation, there will probably result functional failure along certain lines. An example may be given, but as a preliminary, the belief should be stated that a long-continued functional perversion leads invariably to structural change.

Now as to the example:

Your distinguished fellow, Dr. Max Einhorn, in 1893, first described, under the title "Achylia Gastrica," a most important condition of the stomach that was later described by Dr. Allen Jones as "Gastric Anacidity."

Now of the cases reported by the latter, together with several others included in the above-mentioned series, the majority have been examined as to the existence of eye strain. Without a single exception in the cases thus investigated, there has been found to exist a definite and, relatively speaking, uniform ocular defect, viz.: unsymmetrical astigmatism of high degree, varying from one to five diopters. It should be noted that here was an absence of the slight astigmatic faults, such as are reported to be responsible for so much headache. Also, that the present ametropia was irregular, such as myopia in one eye and hyperopia in the other, besides a lack of correspondence in the axes of astigmatism. Obviously no claim is made that these ocular defects are generally followed by "Achylia Gastrica," but it is true that in every case of this form of gastric trouble in which an eye examination was made, ametropia of the forms described was found present.

Letting repose the eye question and asking a moment's attention to the stomach disease under consideration, the opinion is ventured that in "Achylia Gastrica" we have to do with an affection that in the beginning is often a mere functional disturbance. The long-continued inhibition of the peptic glands is succeeded by atrophy, and what is at first an amenable condition terminates in a structural change and is persistent. The history of the patient is

not one of acute or chronic gastritis, but one of long and troublesome functional derangement. These cases have been followed through the very transitional stages, and, as it were, taken *in flagrante delicto*. Here seems to be evidence of the truth of the assertion that long-continued functional disturbance leads to change in structure.

The ætiology of most cases of gastrectasis is involved in the history of functional insufficiency. Many believe that peptic ulcer has often a neuropathic origin. It is highly probable, then, that sometimes as a result of functional disturbance chronic gastritis may be established. The successful management of functional disorders of the stomach, therefore, not only includes an intelligent special treatment of this condition and correction of the diet, as indicated by the precise study of the stomach and its contents, but it includes the discovery and removal of the cause, and that cause is frequently remote from the organ in question.

To recapitulate, it is held that:

1. Functional gastric disorders generally arise from influences outside the stomach.

2. Those causes are usually to be found in some reflex irritation or some toxæmia.

3. Amongst the latter, occasionally syphilis has a place that apparently has passed unnoticed.

4. Structural changes in the stomach are not so much the causes as they are the results of functional disorders.

5. The successful treatment of these affections must include the removal of the often unsuspected exciting cause.

CLINICAL OBSERVATIONS ON EROSIONS OF THE STOMACH AND THEIR TREATMENT.

BY MAX EINHORN, M. D.,
NEW YORK.

As is well known, the term "erosion" signifies a defect of superficial nature. In the stomach erosions are often found at the autopsy. Of late, several valuable papers on the pathological anatomy of this subject and on the rare occurrence of erosions associated with typical ulcers of the stomach have been published.

In his excellent article, "*Ueber geschwürige Processe im Magen*," D. Gerhardt[1] describes erosions of the stomach in the following words: "Sections made of erosions, as a rule, show that at the base of the ulcerations almost the entire lower half of the mucous membrane is still preserved. In the epithelium of these remaining glands, nothing remarkable can be discovered; at the sides the glands become longer; the first ones that are intact usually curve themselves over the defect and partly cover it. The recovery seems to take place by the simple aftergrowth of the gland remnants."

Virchow[2] maintains that besides circulatory derangements in the larger vessels of the stomach (art. coron., gastricæ breves, and gastroepiploicæ) the acute and chronic catarrh of the stomach, especially if the latter is accompanied with vomiting and crampy contractions, are the cause of bleeding ulcers and erosions of this organ.

O. Harttung[3] in his article, "*Ueber Faltenblutungen und hämorrhagische Erosionen*," explains the origin of

[1] Virchow Arch., 1892, cxxvii, 85.
[2] Virchow Arch., Bd. v., p. 363.
[3] Deutsche med. Wochenschr., 1890, No. 38, p. 847.

erosions in the following way: "The contraction of the muscularis of the stomach effects an arrest of the flow in the folds and intense congestion in the veins and capillaries which give cause to hæmorrhages into the mucous membrane. . . . An hæmorrhagic infiltration of the mucous membrane arises, which, therefore, is only little or not at all provided with fresh circulating blood and very soon succumbs to the digestive effect of the gastric juice. In this way the decay and destruction of the tissue, the hæmorrhagic erosion, is produced."

Langerhans[1] opposed the commonly accepted view that between erosion and ulcer of the stomach there is only a gradual but no typical difference, and that ulcers of the stomach very frequently originate from the larger erosions. He gave the following reasons: The round ulcer of the stomach has its typical location near the small curvature, and is met with only exceptionally at other places; the hæmorrhagic erosions, on the contrary, have no predilection in their seat; the round ulcer appears most frequently as a single ulcer, sometimes as two corresponding ones, but very seldom in greater number; erosions, on the contrary, are met with in large numbers, seldom singly. They are located principally at the tops of the folds of the mucous membrane, and flow together, if they are very numerous and near each other, in the form of elongated and ramified figures corresponding to the folds of the mucous membrane; the round ulcer of the stomach loses its characteristic round shape only if two corresponding ulcers at both sides of the small curvature unite to form one oblong ulcer, the longitudinal diameter of which is always perpendicular to the longitudinal axis of the stomach.

Langerhans believes that primary affections of the vessels (aneurisms, embolisms, etc.) are but very seldom the cause of hæmorrhagic erosions, and that, on the other

[1] Virchow Arch., 1891, cxxiv, 373.

hand, catarrhal inflammations, with crampy contractions of the muscularis, only exceptionally cause the formation of chronic corrosive ulcers. Ordinarily, hæmorrhagic erosions appear in crampy contractions accompanied with inflammatory processes; round ulcers of the stomach in primary affections of an arterial vessel. Only very exceptionally do erosions become transformed into chronic ulcers.

Such exceptional cases, in which gastric ulcers very probably took their origin from erosions, are described by Langerhans and Gerhardt in their previously-mentioned papers.

Ewald,[1] speaking of the ætiology of round ulcers of the stomach, mentions also erosions as an important factor, and describes the microscopic picture of a stomach which has been treated according to Heidenhain's method shortly after death. The mucous membrane of this stomach was suffused with blood, and at different places the glandular tubules were filled to beyond their necks with unmistakable red blood-corpuscles. "A very slight local congestion or a traumatism," Ewald remarks at this place, "may be the cause of such bleedings. They develop into the hæmorrhagic erosions, small, streak-like, or rounded losses of substance from the size of a millet seed to that of a pea, on which at times a blackish-brown extravasation of blood is found, together with the simultaneous loosening of the mucous membrane. Their number is very variable, being sometimes enormous, especially near the pylorus, so that the stomach appears as if covered with them."

Whereas the subject in question has been thoroughly discussed and worked at in respect to the pathological anatomy in the aforementioned literature, very little has been done clinically. Although erosions of the mucous membranes of the stomach are mentioned in some text-books, there is nowhere defined how these conditions may be recognized during life.

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[1] C. A. Ewald: Diseases of the Stomach, p. 236, 1892.

The only papers which in a slight measure touch the clinical side of this subject are the following:

Jaworski and Korczynski[1] describe non-dangerous exfoliations of the gastric mucous membrane with traces of bleeding of the stomach caused by the tube; unless they originate from an artificially-ruptured ulcer or cancer, they are never large, and confine themselves to the red coloration of a few portions of mucous membrane, and at the utmost to pink tinging of the washed stomach contents. These small artificial bleedings are different from other hæmorrhages of the stomach; they are fresh and show under the microscope unchanged red blood-corpuscles which are dissipated in the parenchyma of the mucous membrane. These artificial bleedings occur principally in patients suffering from chronic gastric catarrh (lack of HCl secretion), thus frequently in carcinomatous stomachs.

Boas[2] says, in his book on "Diseases of the Stomach": "Sometimes when fasting there appears in the stomach contents also desquamated epithelium and remnants of glandular tubes. They are found oftener than could be concluded from the description given by other authors. In several instances I have observed clear fragments of glandular tubes which could be recognized with certainty." Boas also mentions the occurrence of small exfoliations of the mucous membrane. He speaks of them, however, as something accidental, which could be utilized for the diagnosis by microscopic examination.

Contrary to these described accidental occurrences of pieces of gastric mucous membrane, I have observed during the last two years seven patients in whom the presence of particles of the gastric mucous membrane in the wash-water of the stomach was quite the rule. These

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[1] Deutsche med. Wochenschrift, 1886, No. 49 p. 875.

[2] Boas: Allgemeine Diagnostik und Therapie der Magenkrankheiten, p. 171. Leipzig, 1890.

cases resemble each other in so many respects that they appear as if belonging to one sickness. They could best be considered as erosions of the gastric mucous membrane. It might be appropriate at first to describe these cases as they have been observed clinically, and thereafter to discuss the type of the disease.

CASE I.—June, 1892. M. S——, thirty-four years of age, shoemaker, for the last four years has been troubled with pains after meals. As a rule, the pains are not severe; sometimes, however, although quite seldom, they appear with intensity. Patient feels extremely weak and languid. Appetite ordinarily very poor; bowels constipated.

Status præsens: Strongly-built man; mucous membranes well colored; cheeks hollow. Chest organs intact. Stomach not enlarged, sensitive to pressure, but not specially painful.

The examination of the stomach contents one hour after Ewald's test breakfast, which has been repeatedly done, always showed absence of the free hydrochloric acid, presence of small quantities of combined HCl and ferments; not much mucus.

June 3d.—When fasting: stomach empty; lavage: in the wash-water there are found three red pieces of mucous membrane. (One small piece is examined in glycerin under the microscope, and well-preserved glands found.) The stomach is sprayed with a one per mille silver nitrate solution.

June 5th.—Lavage: three red pieces of mucous membrane appear in the wash-water. Spray with argent. nitr.

June 7th.—Lavage: two red pieces of mucous membrane. Spray with argent. nitr.

June 10th.—Patient feels better. Lavage: no pieces of mucous membrane. Spray with argent. nitr.

June 13th.—Lavage: no pieces of mucous membrane. Spray with argent. nitr.

June 16th.—Lavage: no pieces of mucous membrane. Spray with argent. nitr.

The treatment is discontinued. Patient felt well for almost a whole year, and lived without any complaints whatever.

On July 4th, 1893, patient consulted me anew. He as-

serted that he was troubled with poor appetite, pains after meals, and extreme weakness for the last three to four weeks.

When fasting: stomach empty; lavage: two red pieces of mucous membrane are found in the wash-water, one of them being 1 ctm. long and 0.5 ctm. wide. Spray with argent. nitr.

July 6th.—Lavage: two red pieces of mucous membrane appear in the wash-water. Spray with argent. nitr.

July 8th.—Patient feels considerably better. Lavage: one red piece of mucous membrane. Spray with argent. nitr.

July 11th.—Lavage: no mucous membrane. Spray with argent. nitr.

July 14th and 17th, 1893.—The same condition is noticed during lavage (*i. e.*, no pieces of mucous membrane appear), and the stomach sprayed with argent. nitr. Patient is discharged.

CASE II.—January 12th, 1893. M. G——, aged thirty, merchant, had influenza three years ago. Since that time patient began to suffer from digestive troubles; he used to vomit immediately after meals; patient once noticed in the vomited matter some thick, light-red blood, mixed with mucus (the quantity was one small liquor glass). Patient is troubled, besides, with pains in his back for ten years; these pains increased after the grippe. Poor appetite, no relish of food. Bowels always good. Patient is not much emaciated.

Status præsens: The chest organs do not show anything abnormal. The stomach reaches until about one finger's width above the navel. The gastric region is painful to pressure. The diagnosis made was probable ulcer of the stomach, and the patient was subjected to the ordinary rest treatment. Patient has been fed exclusively per rectum during the first week, and thereupon ordered a milk diet for two weeks. During all this time, warm flaxseed poultices have been applied. During this treatment, and also for some time thereafter, the vomiting stopped, and the pains were less severe when they appeared. Soon, however, patient began to complain anew, and by the middle of February, 1893, all the old symptoms had reappeared.

February 25th.—When fasting: the stomach is washed out (during the lavage of this morning as well as in almost all the following washings, more or less violent coughing and sometimes nausea spells occur). In the wash-water four pieces of mucous membrane (bloody red) are found. Spray with a one per mille nitrate of silver solution.

February 27th.—When fasting, lavage (during this procedure vehement coughing spell): in the wash-water four red pieces of mucous membrane are found; the wash-water has a slightly pink color (admixture of traces of blood). Spray with argent. nitr.

March 1st.—Lavage: in the wash-water four red pieces of mucous membrane; the water is slightly red, especially the last part of it (admixture of blood). Spray with argent. nitr.

March 3d.—Intra-gastric galvanization.

March 5th.—Lavage: several pieces of blood-red mucous membrane. Spray with argent. nitr.

March 7th.—Patient feels better. The stomach is sprayed with argent. nitr.

March 9th.—When fasting, lavage: no pieces of mucous membrane; the wash-water, however, is a trace red. Spray with argent. nitr.

March 11th.—Spray with argent. nitr.

March 13th.—Lavage: one red piece of mucous membrane. Spray with argent. nitr.

March 15th.—Spray with argent. nitr.

March 17th.—Lavage: no pieces of mucous membrane in the wash-water.

March 19th.—Examination of the stomach contents one hour after Ewald's test breakfast; HCl = 0; acidity = 4; no lactic acid; rennet ferment = 0; rennet zymogen +.

March 22d.—Spray with argent. nitr.

March 25th.—Patient complains of having more pains. Lavage: three red pieces of mucous membrane; in taking out the tube, some blood-red water appears. Spray with argent. nitr.

March 27th.—Spray with an ichthyol solution.

March 29th.—Spray with an ichthyol solution.

March 31st.—Patient does not feel any better. Lavage: several red-colored pieces of mucous membrane. Spray with one per cent. alumnol solution.

April 2d and 4th.—Spray with alumnol.

April 6th.—Lavage: only one red piece of mucous membrane. In taking out the tube, some bloody water appears. Spray with alumnol.

April 8th.—Spray with alumnol. As the patient did not improve by the local treatment, the same is for the present discontinued. The patient was given arsenic and iron for some time; his condition, however, did not seem to be influenced by these medicaments.

Besides the pieces of gastric mucous membrane, the wash-water (especially the last part which was emptied when taking out the tube) showed a slight red color. This gave ground to the supposition that the sore spots may be situated at the small curvature, probably not very distant from the cardia. As the patient offered a grave condition (inability to walk, extreme weariness, pains in the gastric region as well as principally in the back in the neighborhood of the seventh vertebra), it appeared to me (and also to one of the best surgeons in this city) that an operation (exploratory laparotomy), with the intention of an eventual excision of the erosions (if there should be only few of them) would possibly give some benefit.

The patient left for Germany, and consulted several colleagues, but none advised an operation; he was sent to Kissingen, where he passed six weeks without any benefit, and finally returned to New York with his old complaints.

The examination of the patient on January 6th, 1894, showed the conditions unchanged: When fasting, lavage: three red pieces of mucous membrane appear in the wash-water; one of the pieces is 1 ctm. long and 0.4 ctm. wide, and shows several dark red ecchymoses of the size of the head of a pin; several microscopic specimens have been prepared, some in the fresh condition, the others dried and stained; one can easily see the glandular tubes and certain places of group-wise accumulations of red blood-corpuscles, the latter being of a slightly red color.

CASE III.—February 11th, 1893. H. S——, aged thirty-five, merchant, suffers for two to three years from digestive troubles. These consist principally in the appearance of pains right after meals; the pains are not severe; they produce, however, the effect that patient eats less. There

is a feeling of fulness; bowels constipated. Patient always feels weak and tired.

The examination reveals: chest organs intact; the gastric region is sensitive to pressure; there is splashing sound extending two fingers' width below the navel; right kidney movable.

The examination of the stomach contents one hour after Ewald's test-breakfast showed: HCl +; acidity = 60.

February 13th.—When fasting: stomach empty. Lavage: in the wash-water three small red pieces of mucous membrane are found. Spray with argent. nitr.

February 14th.—Intra-gastric galvanization.

February 15th.—Lavage: in the wash-water three small red pieces of mucous membrane appear. A fresh microscopic specimen shows gastric glands. Spray with argent. nitr.

February 16th.—Patient feels better,—*i. e.*, he is stronger, can eat more, and is not troubled with pains. Direct galvanization of the stomach.

February 17th.—Lavage: no pieces of mucous membrane are found. Spray with argent. nitr.

February 18th.—Intra-gastric galvanization.

February 19th.—Lavage: no pieces of mucous membrane. Spray with argent. nitr.

February 20th.—Intra-gastric galvanization.

February 21st.—Examination of the stomach contents one hour after the test-breakfast: HCl +; acidity = 54; no pieces of mucous membrane.

February 22d.—Direct galvanization of the stomach.

February 23d.—Lavage: no pieces of mucous membrane. Spray with argent. nitr.

February 24th—Intra-gastric galvanization. Patient had to return to his native city, Chicago, on account of urgent business. As I have recently heard, patient felt well all the time, with but few intervals.

CASE IV.—April 24th, 1893. L. A——, aged thirty-three, merchant, suffered for two to three years from temporary attacks of pains right after meals. The pains are not of a severe nature; they, however, have a depressing effect on patient and cause him to partake of much less food than he is otherwise used to. The bowels are only slightly costive. The periods of pains extend over about

two to three months, and are then followed by periods of complete euphoria. For a month the patient has pains after meals; the appetite is poor, and frequently a feeling of extreme weakness overcomes the patient.

Status præsens: Chest organs do not reveal anything abnormal. The gastric region is sensitive to pressure, but there is no distinct pain. After the partaking of half a glassful of water, a splashing sound can be produced extending to two fingers' width above the navel.

April 27th.—The stomach contents are examined one hour after the test-breakfast: HCl +; acidity = 30.

Patient was given nux vomica and gastro-faradization three times, but without showing any improvement.

May 9th.—When fasting, lavage: four red pieces of gastric mucous membrane appear in the wash-water. Spray with a one per mille nitrate of silver solution.

May 11th.—When fasting, lavage: four red pieces of mucous membrane are found in the wash-water. Spray with argent. nitr.

May 13th.—Intra-gastric galvanization.

May 15th.—When fasting, lavage: one piece of mucous membrane is found in the wash-water. Spray with argent. nitr.

May 17th.—Intra-gastric galvanization.

May 19th.—When fasting, lavage: no pieces of mucous membrane in the wash-water. Patient feels much better and leaves for the country.

CASE V.—April 19th, 1893. B. M. S——, aged twenty-six, merchant, complains for two and a half years of digestive troubles. At first, patient had lack of appetite, pain after meals, and nausea, but no vomiting. Feeling of weariness and fatigue; constipation. After some continued treatment and a trip to the South the condition of the patient improved for a while; soon, however, it got worse again. During the last two years, patient has constantly pains right after meals, with but very few exceptions, and feels very weak. When fasting, patient, as a rule, feels well.

Status præsens: Chest organs intact; the gastric region is sensitive to pressure. After drinking half a glassful of water, a splashing sound can be produced, extending to one or two fingers' width above the navel. Liver not enlarged. Urine contains neither sugar nor albumin.

April 20th.—Examination of the stomach contents one hour after Ewald's test-breakfast shows: HCl +; acidity = 60; admixture of much mucus.

Diagnosis: Gastritis gland. chron. mucosa.

April 21st.—When fasting, stomach empty, lavage: in the wash-water, three red pieces of gastric mucous membrane. (A fresh specimen in glycerine shows gastric glands.) Spray with argent. nit.

April 23d.—Intra-gastric galvanization.

April 25th.—Lavage: three red pieces of mucous membrane appear in the wash-water. Spray with argent. nitr.

April 27th and 29th.—Direct galvanization of the stomach. Patient had to leave New York on account of business and returned on May 17th.

May 18th.—When fasting, stomach empty, lavage: three red pieces of mucous membrane are found in the wash-water. Spray with argent. nitr.

May 20th.—Intra-gastric galvanization.

May 22d.—Lavage: two red pieces of mucous membrane are found. Spray with argent. nitr.

May 24th.—Patient feels better, has a better appetite, and hardly any pain. Lavage: no pieces of mucous membrane are found. Spray with argent. nitr.

May 26th.—Direct galvanization of the stomach.

May 30th.—Lavage: no pieces of mucous membrane. Spray with argent. nitr.

June 2d.—Intra-gastric galvanization. Patient feels well and is, therefore, for the present dismissed.

CASE VI.—August 24th, 1893. G. B——, aged thirty-two, merchant, complains of digestive troubles for about ten years. Appetite is seldom good; after meals patient has a slight painful feeling in the gastric region. Bowels almost always constipated. Off and on there appears a feeling of extreme fatigue to such a degree that patient is hardly able to walk. There was never vomiting, but very frequently much belching. Flat taste in the mouth, and, at times, highly increased salivation. Patient has been in Carlsbad several times, but without any benefit.

Status præsens: Chest organs intact. The gastric regions sensitive to pressure, but not considerably painful. Splashing sound after the partaking of half a glassful of water, extending to three fingers' width above the navel.

August 26th.—The examination of the stomach contents one hour after Ewald's test-breakfast shows: HCl = 0; acidity = 24; Rennet ferment +; very much viscid mucus.

Diagnosis: Gastritis gland. chron. mucosa.

Treatment: Strychnia and iron; besides extr. fluid. cascar. sagrad.

Patient then left for his native city, Altoona, but returned to New York at the beginning of November, in order to undergo treatment.

November 2d.—When fasting, stomach empty, lavage: in the wash-water, much mucus and two red pieces of gastric mucosa. Spray with argent. nitr.

November 4th.—Direct galvanization of the stomach.

November 6th.—Lavage: two red pieces of mucous membrane. Spray with argent. nitr.

November 8th.—Patient feels better; he can eat more and feels stronger. The examination of the stomach contents one hour after the test-breakfast shows: HCl + ; acidity = 52.

November 11th.—Direct galvanization of the stomach.

November 14th.—Lavage: no pieces of mucous membrane found. Spray with argent. nitr.

Nov. 17th.—Intra-gastric galvanization.

Nov. 19th.—Lavage: no pieces of mucous membrane. Spray with argent. nitr.

November 22d.—Direct galvanization of the stomach.

November 24th.—Lavage: no pieces of mucous membrane. Spray with argent. nitr.

Nov. 26th.—Intra-gastric galvanization.

Patient feels better and returns home.

CASE VII.—May 20th, 1893. J. J. M——, thirty-six years of age, pharmacist, began to suffer from digestive troubles five years ago. Appetite was very poor. Patient ate only, as he expressed himself, "from habit." Tongue thickly coated. Frequently dizziness and darkness in front of the eyes. This latter symptom often appeared so violently that the patient had to stop work for fifteen to twenty minutes, in order to recuperate. Fatigue and general weakness. Bowels always constipated. Feeling of pressure in the gastric region, only seldom increasing after meals.

Besides these general symptoms, patient two to three times yearly had attacks growing worse. These attacks were as follows: at night patient fell asleep with the feeling of extreme fatigue; at 2 a. m. he used to wake up with a strong irritation in the throat and in the œsophagus and spat constantly thick mucus. This condition (*i. e.*, the uninterrupted spitting) used to last frequently until 4 a. m. or 4:30 a. m., and patient nearly always was obliged to provoke vomiting by tickling his throat in order to get relief. These attacks of deterioration used to last three to six weeks. During such an attack patient came under my care.

Status præsens: Slender man with very thin panniculus adiposus; cheeks somewhat hollow. Chest organs intact. The gastric region is somewhat sensitive to pressure, but no intense pain can be produced at any place. There is a splashing sound extending to one to two fingers' width above the navel. Tongue heavily coated. Knee-jerk present, perhaps somewhat increased. Patient can stand with eyes closed. Urine does not contain any sugar or albumin.

The examination of the stomach contents on May 21st, one hour after Ewald's test-breakfast, showed: HCl +; acidity = 96; lactic acid not present.

Diagnosis: hyperacidity. Treatment: alkalies; richly albuminous diet.

May 23d.—When fasting, stomach empty, lavage: in the wash-water, three red pieces of gastric mucosa are found. Spray with one per mille nitrate of silver solution.

May 25th.—Intra-gastric galvanization.

May 27th.—Lavage: in the wash-water, two red pieces of mucous membrane. (One small piece examined under the microscope in the fresh state shows gastric glands.) Spray with argent. nitr.

May 29th.—Direct galvanization of the stomach.

May 31st.—Lavage: no pieces of mucous membrane are found. Spray with argent. nitr.

June 2d.—Direct galvanization of the stomach. Patient has a good appetite and feels much stronger. The feeling of pressure has disappeared and sleep is good. Further treatment is therefore dispensed with.

In summing up all the cases described, one is confronted with the following striking points: In washing the stomach when the patient is in the fasting condition, one to four small pieces of gastric mucous membrane are found. They are about 0.3 to 0.4 ctm. long and nearly as wide, and present a blood-red color. Under the microscope, one sees well-preserved glands and accumulations of red blood-corpuscles. These pieces of gastric mucosa are constantly found if the stomach of the patient is washed out in the fasting condition. We have not to deal here with an incidental lesion caused by the tube; for, on the one hand, the lavage has been performed without any aspiration and by means of a soft tube; on the other hand, one could not observe in a casual lesion that constancy which is found here.

Besides this objective point in the patients mentioned, several subjective symptoms appear with particular clearness, namely, pains, emaciation, feeling of weakness.

The *pains*, as a rule, are not intense, and make their appearance right after meals, no matter what has been eaten. They last various lengths of time (one to two hours), and then gradually disappear. Complaints of severe attacks of pain we have never met with in these cases. Frequently there appear, for periods of different lengths, intervals perfectly free from pain, combined with a feeling of complete euphoria. Only seldom do the patients feel pain all the time without reference to food.

Emaciation.—Most cases observed lost in flesh during the first period of their sickness, but thereafter kept up their weight quite constantly. They looked rather thin (in the face the jaws protrude, the cheeks are thin and somewhat hollow), but did not, however, present that cachectic color we are accustomed to meet in carcinoma and other grave chronic troubles.

Feeling of weakness.—All the patients complained of a feeling of lassitude, weakness, lack of ambition, and

inability to work, and of a decrease of bodily strength. This symptom appears most markedly right after meals, and decreases somewhat a little while afterward (one-half to one hour). In one of my patients (G. B——) there usually appeared, once in a week or in a fortnight, an acme of this symptom, associated with complete anorexia, and lasting for about two days. During this period of deterioration, patient was hardly able to walk.

What is the condition of the gastric juice in the cases described?—In the first six, one encounters a decrease in the HCl secretion and in the acidity of the stomach contents. In two of these cases (G. B—— and B. M. S——), there was always found a considerable amount of mucus. In all these cases, the diagnosis has been made: "Gastritis, gland. chronica;" in the two last, "Gastritis gland. chron. mucosa." In the seventh case (J. J. M——), however, there was found superacidity caused by an increased HCl secretion.

How are we to explain the constant occurrence of the small pieces of gastric mucosa?—In six of the cases narrated, blood was never found in the wash-water carrying the small pieces of mucous membrane. Only in one patient (M. G——), the wash-water often used to have a very slight red color. In this case, coughing spells frequently appeared during lavage, and in such an instance, besides the pieces of gastric mucosa, the water was stained slightly red.

The pieces of gastric mucosa found in the wash-water of our patients probably must have partly or wholly peeled off from the mucous membrane of the stomach some time previous to the washing. This would explain why there is no bleeding during the lavage. The spots on which the exfoliations have taken place and thus present "erosions," might explain the soreness met with in our patients. One can also easily understand the appearance of blood from

the sore spots caused by violent contractions of the stomach during a coughing spell (in case M. G——).

It is very difficult, or perhaps at present hardly possible to decide, whether the exfoliations always take place at the same spots—the mucous membrane constantly becoming replaced and peeled off again—or whether the whole (or a great part) of the inner surface of the stomach is affected in such a manner that small pieces of mucosa easily peel off here and there. This question can only be answered after a long study of vast clinical and pathologico-anatomical material. These exfoliations take place (whether always on the same or on different spots) day by day in the stomach of our patients, and effect temporary erosions.

In all cases mentioned, similar in so many points, one constantly encounters the exfoliations of the gastric mucosa, respectively the resulting erosions. Therefore this pathological condition might be most suitably termed "erosions of the stomach."[1]

Ætiology.—In the greatest number of cases mentioned, there was found chronic gastric catarrh associated with erosions. The same probably gave cause to the origin of the latter anomaly. Some cases of erosions of the stomach may, however, be caused by some factors yet unknown.

Course.—The course of this pathological condition is a very prolonged one. Several of the patients appear to have suffered from it for many years. Although there may be intervals of perfect euphoria (at the same time probably the inner layer of the stomach is completely intact) for a longer or shorter period of time, the old symptoms do, however, sooner or later return.

One would imagine that cases of erosions of the stomach

[1] REMARK.—We prefer the term "Erosions of the stomach" instead of the more correct but longer name, "Erosions of the gastric mucosa," as there is no fear of any misunderstanding.

would present a very fruitful soil for the development of ulcers. This, however, does not seem to be the case, for in none of the patients (with the only exception perhaps of M. G——) was there any justifiable supposition of an existing ulcer during the long course of the sickness.

Treatment.—The local treatment of the stomach here plays a great rôle. The astringent effect of nitrate of silver solutions in similar more accessible affections led me to apply this substance directly to the inside of the stomach. This can best be achieved by means of the spray. It was on this occasion that I constructed the gastric spray apparatus, and recommended its use in the field of diseases of the stomach.[1]

Gastric Spray Apparatus.

The treatment is given in the following way: First, the stomach in a fasting condition is washed out with lukewarm water; when all the water has been emptied, the tube is removed from the stomach. The spray apparatus is filled with 10 c. c. of a 1 to 2 per mille solution of nitrate of silver, the tube end dipped into warm water and inserted into the stomach (length of tubing, 50 ctm.); thereupon the whole, or at least the greater part, of the solution in the bottle is sprayed; the bottle is then opened and the spray tube removed from the stomach.

[1] New York Medical Journal, 1892–lvi–326.

In fact, the good result of this method of treatment can frequently be best shown in the affection in question, for after the spraying has been done several times, the small pieces of gastric mucosa cease to appear. Associated with the objective symptom, there appears an amelioration in the subjective feeling of the patient; the pains grow considerably less or entirely disappear, and the strength increases.

I usually combine the spray nitrate of silver treatment with intra-gastric galvanization, alternately applying the spray or the galvanization. The reason for the use of galvanization in these cases lies in the fact that I had such effective results in two other cases of probable erosions of the stomach, complicated with heart trouble,[1] by means of galvanization alone. The methodical application of intra-gastric galvanization combined with the spray seems to increase the curative effect.

As to diet, there is no need for being very rigorous in these cases. Frequent meals, avoiding heavy vegetables, salads, and pastries, is all I ordinarily require.

Cold ablutions, light gymnastics, out-door life are to be warmly recommended.

Of medicaments, condurango and nux vomica are frequently, and a good, easily assimilated iron preparation always, appropriate.

Although these medicaments may be of value as adjuvants, we should rely, in my opinion, mainly upon the local treatment.

DISCUSSION.

DR. F. P. KINNICUTT.—I have only a few words to say in regard to Dr. Einhorn's paper, which interested me greatly. The subject is certainly deserving of careful study. I confess that my attention was never called to the presence of these shreds of the mucosa of the stomach in

[1] New York Medical Journal, 1893–lviii–29.

the fluid that is withdrawn after washing the organ, until Dr. Einhorn spoke to me about them about two weeks ago. I told him then, that while I had in some instances observed such shreds in the washings, I attributed their presence to some injury done to the gastric mucous membrane. In one case, I observed a piece of mucous membrane quite as large as those described by the doctor in his paper, and I supposed its presence was due to accidental injury to the mucosa. Dr. Einhorn presents the subject under a different aspect, and I shall bear his observations in mind.

His method of treating these cases is very ingenious. While I consider it good treatment, still the statement made by him, that the rapid improvement was largely brought about by the local applications, that is, spraying the stomach with a solution of silver nitrate, must be held under reservation.

DR. W. P. NORTHRUP.—I should like to ask Dr. Einhorn whether in any of his cases there was a history of chronic ulcer of the stomach, preceding or following the erosions, or whether there was any history of carcinoma. I cannot recall any cases occurring in my practice where there was an exfoliation of the whole mucosa of the stomach. In children, we meet with cases where layers of mucus become entangled in the superficial cells of the lining membrane of the stomach or colon, associated with a swollen condition somewhat similar to a superficial pseudomembrane; but in none of these instances was the whole glandular structure involved. I can understand how the superficial layers of the mucosa, involving the openings of the glands, should peel off in much the same way as the epidermis of the skin may exfoliate, but I cannot understand how these changes can extend down to the submucosa.

DR. J. FUHS.—I quite agree with the statement made by Dr. Stockton, that functional disturbances of the stomach may be due to a variety of causes, affecting either the stomach itself or some distant organ. It is of primary importance to understand the nature of the disturbance, and this is not always an easy matter. In cases where there is an excessive secretion of hydrochloric acid, there may also be a disturbance of the motor function of the organ. In one such case, for instance, if the stomach

be examined one hour after a test meal, it may be found entirely empty; in another case, where this same hyper-secretion exists, the stomach may contain a considerable quantity of food two hours or longer after a test meal. The treatment in these cases must be directed not only towards a correction of the hyper-acidity, but also towards a correction of the motor disturbance. Some of these cases must be kept under observation for a prolonged period before a correct idea of the condition of affairs can be arrived at, for the conditions in the same case may vary from time to time.

There is a certain class of cases with functional gastric disturbance where we can say almost with certainty that the latter is due to a diseased condition of an organ distant from the stomach—I refer to diseases of the pelvis of the kidney. A number of such cases have come under my observation, and in all of them the symptoms have corresponded very closely. These patients at first invariably referred their symptoms to the stomach; there was nothing to indicate that the primary seat of the disorder was in the kidney. An analysis of the urine gave negative results. An examination of the stomach contents revealed diminished acidity; there was no disturbance of the motor function. After a time the patients complained of severe headaches, chiefly confined to the occipital region, with some nausea. They also suffered from excessive burning in the epigastric region. They would have occasional attacks of vomiting, the material vomited containing a very high percentage of hydrochloric acid ($4\frac{0}{00}$). The evacuation of this fluid would be followed by a moderate amount of relief; then the same symptoms would recur. Shortly after, the urine became charged with pus, and with its discharge the stomach symptoms were relieved.

I have seen two cases which might possibly be classed among those described by Dr. Einhorn. In one of them, on two occasions, a piece of mucous membrane was found in the material removed from the stomach; there was no bleeding. In the other case, the patient would vomit an alkaline fluid, slightly tinged with bile, and containing shreds of gastric mucous membrane. In these, the arrangement of the tubules could be made out. The symptoms in these two cases did not correspond. One was a lady, who

was greatly emaciated, and who suffered from pain in the gastric region after eating; this pain was aggravated by excitement, and was often accompanied by herpetic eruptions. There was no excessive acidity of the stomach contents. The other case was that of a man who also complained of pain at the epigastrium after his meals; in that case there was an excessive secretion of HCl.

THE PRESIDENT.—It is not often a source of regret that papers read at the Academy are not longer, but in this case I am certainly sorry that Dr. Stockton did not enter more fully into the point referred to in his paper that functional diseases of the stomach sometimes depend on what is called "eye strain." We have all heard that almost every possible ailment may be dependent on a condition of the eye. This is very little understood and in my opinion it exists with comparative infrequency. But generally, when we hear about it, it is with reference to what are called muscular anomalies, or want of proper coördination—want of equilibrium in the external muscles of the eye. It is hardly necessary to tell this Academy that some of us do not believe at all in any such thing as want of muscular equilibrium of the eye being a causative factor in the production of constitutional disease. Dr. Stockton, in the reference made to his cases, speaks very distinctly of a fixed static condition, namely, astigmatism of a high degree. I am sorry to say there is very little known in the general profession about the relative frequency of the normal eye. Let us take, for instance, one hundred people, who have never had any trouble with their eyes, so far as they know—certainly, no marked trouble—who are not neurotics, nor dyspeptics, nor afflicted with any constitutional disease. If any one will take the trouble to examine the eyes of these people, as some of us have done, he will be surprised to learn how very few of them possess what might be called the standard eye. If the eye is responsible for any functional disturbances, I think their origin will be found, not in lack of muscular equilibrium, but in these fixed static conditions, such as astigmatism, but even these cases are extremely rare. Scarcely ever, in my opinion, does a human being have a reflex trouble from his eyes without being conscious of its origin. If a patient comes in to see you suffering with epilepsy, or chorea, or

phthisis, and you have in your mind the idea that the cause of his constitutional trouble is a lack of proper equilibrium of the eye muscles, ask that person if he has any ocular trouble. If an examination decides this in the negative, then the epilepsy or other trouble is not due to reflex conditions.

Cases of epilepsy have been due to hardened wax in the external auditory canal, but when this was the case, there was impairment of hearing, a sensation in the ear which called attention to it, and just so, every real reflex trouble will show a starting point in the organ from which it is reflected.

To the discredit of the profession in this country, this subject of reflex conditions due to the eye, has been discussed to a ridiculous degree. A broken thigh and astigmatism are discovered in the same person: therefore, the broken thigh is due to the astigmatism. That, roughly speaking, is the character of the logic that is sometimes submitted to us. The human eyeball will tolerate no end of deviations from the normal without reflecting any trouble on the constitution. Be very sure of your ground before you assume that what is a part of the inheritance of the human race in the vast majority of cases has anything to do with the causation of certain forms of constitutional disease. About twenty per cent. of the human race suffer from myopia; about seventy-nine per cent. have hypermetropia and astigmatism, and a bare one per cent. possess the ideal eye. I do not wish to imply, however, that organic and deleterious conditions of the eyeball, such as a high degree of astigmatism, may not produce constitutional symptoms.

DR. C. G. STOCKTON.—I have very little to say in closing. I wish to thank Dr. Fuhs for his remarks, and quite agree with his statement that each case of functional gastric disturbance demands special treatment, and that in many instances the treatment must be directed to the stomach, *per se*. Some patients will require treatment throughout life and yet those cases might be relieved, in a certain proportion of instances, if we could locate the origin of the trouble which keeps up the irritation of the stomach, and which, in my judgment, is often remote from the stomach. This conclusion has been growing on me for some years;

I have given it much thought, and I considered it of sufficient importance to be spoken of. I feel very grateful indeed that my reference to the eye, in this connection, brought out such an interesting discussion from your president. On the subject of the relation of the eye to stomach diseases I am simply a student. I studied this matter because I have heard very much said on both sides of the question, and I have fully satisfied myself that it is worthy of attention, at least. I make no other assertion to-night, however, than that in the cases referred to there was present pronounced astigmatism. Whether this ocular condition led to the gastric disturbance or not is a matter which can be determined only by further observation. I must differ from the views expressed by your president regarding the bearing of errors of refraction upon disturbances of the general health. Possibly Dr. Roosa is more interested in matters pertaining to the patient's vision than to his general health. I consider the general health of more importance than the vision. I look on the eye entirely from the standpoint of the general practitioner. I have often seen those symptoms which are commonly classed under the name neurasthenia markedly relieved by a correction of refractive errors. Of course, we all know that some nonsense has been written on this subject. I do not believe that epilepsy and phthisis owe their origin to eye strain, but I do believe that many constitutional disturbances, perhaps minor in character, have such an origin.

DR. EINHORN.—Functional disturbance of the stomach is a subject about which we do not, as yet, know very much. Many observations have been recorded regarding it, but we have not yet reached definite conclusions. Certainly some cases of functional gastric disturbance owe their origin to organs distant from the stomach. Which organ is the primary cause of the disorder is sometimes difficult to decide. I agree in the main with the statements made by Dr. Stockton.

In the discussion of my paper, Dr. Kinnicutt expressed the view that the pieces of mucous membrane found might be artificial products. This also occurred to me at first, but I have examined so many of the specimens, and they were found in certain cases with such regularity, that I soon abandoned that view. As regards the value of the

spray in the treatment of these cases, Dr. Kinnicutt said that it is possible that these patients might have recovered just as rapidly without local applications. In reply to this I can only say that before I began to use the spray, my results in the treatment of these cases were not nearly so satisfactory. In one case reported in my paper I had no success whatever with the spray.

To Dr. Northrup's question as to whether the entire mucous membrane comes away, I cannot give a positive answer at present. In the fresh condition, it is very difficult to examine these specimens closely. I was simply satisfied to find that it was mucosa. In one of the specimens there was a small piece of the submucosa. From a study of my own cases I should conclude that these erosions of the stomach have no connection whatsoever with ulcer or cancer of the organ. In six of my cases there was no history of ulcer whatsoever; in the seventh case, the patient gave some symptoms which pointed in that direction, but not with certainty.

In reply to Dr. Fuhs's remarks I would say that we cannot make a diagnosis of this condition by the occasional presence of a piece of mucous membrane in the washings from the stomach; it is only from the constant repetition of this occurrence that we can conclude that we have such a pathological condition to deal with.

Stated Meeting, June 7th, 1894.

TEN YEARS' EXPERIENCE WITH ALEXANDER'S OPERATION FOR SHORTENING THE ROUND LIGAMENTS OF THE UTERUS. SIXTY-FIVE OPERATIONS.

By PAUL F. MUNDÉ, M. D.

Soon after the publication of Alexander's little book on the operation revived, and, I believe, first actually performed by him, I, on December 12, 1884, undertook it successfully, following closely the directions laid down in Alexander's work. I was so much impressed with the ease of performance and the immediate and subsequent results of the operation, that, during the next two years, I had six cases to report, in four of which I succeeded in finding the ligaments, in drawing them out, and attaching them as recommended by Alexander. My experience with the operation increased so rapidly that in the *American Journal of Obstetrics* for October, 1888, I was able to report twenty-three cases, with but three failures to find the ligaments, and with practically no failure in the other twenty cases as regards the permanency of the result of retaining the replaced uterus in the normal position. In that article I reviewed at some length the indications, results, and technique of the operation, and concluded by raising my voice very forcibly in its favor as against the later, more dangerous, less logical, and permanently less successful operations of shortening of the round ligaments through the opened abdominal cavity, or ventral fixation of the fundus uteri.

That article was not read before any medical society, but was simply printed in the above-named journal, where, of course, it was seen by very many members of the profession, and perhaps did not attract as much attention as if it had been more widely circulated through the columns of the weekly medical press, or through the "Transactions" of the medical society before which it might have been read. Hence I am afraid that its precepts and deductions were not so generally appreciated as they should have been. Of course, I can speak only for myself and my own experience, and cannot expect that my professional brethren will unhesitatingly accept and follow my conclusions. But as I am known to be a conservative operator, not only in cœliotomy for diseased ovraries and tubes, but also as regards other operations which may possibly be avoided, I felt that I was entitled, with all due modesty, to more confidence and belief than if the operative furor had had exclusive possession of me. Nevertheless, I am constantly meeting gentlemen who, with quite as much experience as my own in all sorts of gynecological operations, still express a want of reliance in Alexander's operation, and a preference for abdominal section with intra-abdominal shortening of the round ligaments or ventral fixation for the very same objects for which I unhesitatingly prefer Alexander's operation. It seems very difficult to convince some men that pelvic diseases in the female can be treated otherwise than by opening the abdominal cavity, and that, after all, in many cases this should be the last resort when all other possible means have been exhausted.

Some of these gentlemen base their objections to Alexander's operation on the difficulty of finding the ligaments or the impossibility of drawing them out; others, again, contend that the results of the operation are not permanent; and others, again, claim that cœliotomy is a so much easier and equally safe way of attaining the same object. It is scarcely necessary for me to say that I am perfectly

familiar, from a theoretical and practical experience in gynecology of over twenty-five years, with the various methods which are preferred to Alexander's operation by the gentlemen to whom I refer. I do not prefer Alexander's operation because I have a particular hobby in its direction or because I have anything other to gain by my preference than supporting what I think to be best. I can simply point to an experience which has now amounted to sixty-five operations, and which, with the increased knowledge of years, still leaves me a more and more confirmed supporter of the operation.

For minute details as regards the technique and the indications, I will refer my readers to the article published in the October, 1888, number of the *Journal of Obstetrics*. I will add but a few brief remarks as to what is stated there. First, as regards the technique of the operation, I will repeat that if the ligaments are not found it is always the fault of the operator. It is simply a question of careful attention to the anatomical landmarks, namely, the spine of the pubis and external inguinal ring, upon which depends the finding of the ligaments. If the tissues are quickly divided down to the fascia covering the external inguinal ring, this is lightly picked with the scalpel, the knuckle of fat which pops out through the opening is carefully dissected out bluntly with the handle of the scalpel, always being careful to avoid detaching the bundle of fat from the spine of the pubis; if then this bundle of fat is entirely lifted up from the subjacent bone with mouse-tooth forceps or an aneurism needle, a slow, minute, and blunt separation of the tissues thus lifted up will always succeed in revealing the initial fibres of the round ligament, which usually lie at the lower surface of the bundle thus lifted up, and run slightly outward and downward, not entirely outward. If traction upon the supposed fibres of the ligament shows that it runs toward the anterior superior spinous process of the ilium, the operator may be sure that he has

picked up fibres of the external pillar of the ring or of the oblique fascia. Careful isolation of these fibres, gentle traction upon them if they prove to run in the right direction, and dull separation of the strand if it appears attached in the canal, will usually in a few minutes enable the operator to draw out the ligament so that its identity is at once apparent both to him and the spectators. This point having been reached, it is merely a question of gentle, continued traction to draw out as much of the ligament as is allowable, that is to say, until its increasing size, usually almost to that of a lead pencil, shows that its attachment to the uterus is approaching. If traction is too severe and too rapid, or if the ligament is either thin or much adherent, it may easily be broken, particularly in its lower two thirds, and may then be lost to future research, or, if found again, will have to be picked up within the depth of the canal. I confess that I still have the impression which I had years ago, that this operation requires all the wit and dexterity of the operator in order that by some oversight or carelessness the ligament may not be lost, broken off, or pushed aside and thus lost.

The one unvarying landmark is always the spine of the pubis, where I have repeatedly succeeded in picking up the ligament, which at first I had failed to isolate in the bunch of fat lifted up according to direction. By non-attention to this landmark the operator may get to one side and manage to pick up some other cord, even drawing it out to a small extent, as in one case operated on by one of my house surgeons, until I, coming to the rescue, satisfied myself and him that it was the femoral artery, which of course was very hastily dropped, fortunately with no evil results. By returning to the spine of the pubis and proceeding thence forward, I in a moment, picked up the terminal fibres of the round ligament, isolated them, and drew out the ligament.

There will always be in this operation one drawback,

and that is the possibility that the thinness of the ligaments, their very diffuse attachment to the spine of the pubis, and their uncertain appearance in the suprapubic fat, further, the adhesion of such a thin ligament in the Nuckian canal, may result in failure, either through the inability of the operator to isolate them or in their breaking during his attempt to draw them out. In my last case the ligament on the left side appeared to be unusually friable, for it broke close to its uterine origin, the only instance where I have seen it torn so high up. By passing the first suture very deeply I managed, however, to keep the left horn of the uterus close to the anterior abdominal wall and the result was perfect. This accident, the breaking of the ligaments, is, in my experience, the only drawback which this operation possesses. And I think by extreme care in traction it also can often be avoided.

I always stitch as much of the ligament as is not drawn entirely out of the external inguinal ring into the canal by passing a number of silk-worm-gut sutures through each pillar of the ring and the ligament between, cutting off the superabundant portion of the ligament. I have very seldom found it necessary to split up the inguinal canal to the internal ring to find the ligament, and then only when the ligament had been torn off during my first attempts at traction. I can see no use in opening the whole inguinal canal at first to perform the operation. It strikes me like a confession of timidity at the very outset to see this done. I am sure that out of the sixty-five cases which I have performed I have not been obliged to open the whole inguinal canal in more than half a dozen cases. The usual length of my incision is not more than two inches.

Suppuration has occasionally resulted, particularly if much tearing and manipulation were required to find and draw out the ligaments. I formerly employed a split bone drainage-tube, but have for some years entirely discarded it, and cannot say that my cases have done any worse.

Even if suppuration did take place in the wounds, the adhesion of the internal end of the ligament seemed to be quite firm enough to keep the uterus in position.

None of my patients have ever shown the least dangerous symptoms in consequence of the operation, although several times convalescence was prolonged to four and six weeks in consequence of suppuration. Never have I seen an inguinal hernia follow the operation. On the other hand, I have cured an inguinal hernia by the operation. I have always introduced a pessary into the vagina immediately after the operation, in order to prevent undue strain upon the newly attached ligaments, and have had it worn for from three to six months. Of course if perineorrhaphy was done at the same sitting a pessary could not be used.

As regards the length of time required to perform Alexander's operation, I would say that much depends upon the character of the case. In an easy case without adhesions I have frequently succeeded in completely drawing out both ligaments in from five to ten minutes for each side. The stitching, of course, must be carefully done, and the whole duration of the operation will probably not be less than thirty minutes. I need scarcely say that it is always imperative to draw out both ligaments, since to shorten only one ligament would result in placing the uterus in an oblique position.

Second. As regards the indications for the operation, I would say that I consider the lines very closely drawn. Only a sharply retroverted or retroflexed uterus, with more or less descensus, with more or less relaxed vaginal walls, with perfect mobility of the uterus and adnexa, justifies Alexander's operation. I say justifies, but I do not necessarily mean that these conditions are an absolute indication for the operation, since in many such cases perfect relief may be given the woman by a properly adjusted pessary. It is only when a pessary cannot be fitted or comfortably worn, which will retain the uterus and ovaries in their

normal position, or when the patient insists upon being cured instead of being merely temporarily relieved by a pessary, that we can say that Alexander's operation is indicated.

Whenever the uterus or appendages are adherent, Alexander's operation is absolutely and positively contra-indicated. We can, therefore, not compare the indications for Alexander's operation and those for ventral fixation, in my opinion, since the latter is justified and indicated only when adherence of the appendages or of the uterus warrants our opening the abdominal cavity and detaching the adhesions. Then, of course, it is scarcely worth while to do an Alexander's, since it adds very little to the danger to sew the uterus to the anterior abdominal wall. In such cases of adherent uterus and appendages, however, where they have been detached through an abdominal incision, the operation of shortening the round ligaments by doubling them upon themselves, as recommended by Wylie, A. P. Dudley, Mann, and others, should be considered. I have but one experience with this operation, which is still too recent to enable me to offer a positive opinion on the subject. Technically, it is exceedingly pretty, logical, and easily effected. In prolapsus uteri I am not in favor of Alexander's operation, except in so far as it is one in a series of operative measures by which a retention of the uterus in its normal position in the pelvis is to be attained. Alone, Alexander's operation is, in my opinion, of no permanent benefit for prolapsus uteri. Neither do I consider it applicable to cases of anteversion and anteflexion in which it is thought advisable to lift up or straighten out the fundus uteri by drawing it up. In prolapsus uteri my experience is that the round ligaments are not able to withstand the constant downward strain upon them of the large body of the uterus. In anteversion and anteflexion the direction of elevation of the fundus produced by shortening the round ligaments would be such as to drag the

fundus against the symphysis pubis and produce uncomfortable pressure on the bladder.

Third. In a very large proportion of cases where Alexander's operation is indicated, there exists, besides the retroversion and retroflexion and descensus of the uterus, a want of tone or an absence of the inferior uterine and vaginal supports, namely, the perineum is lacerated, the bladder and anterior vaginal wall and the rectum and posterior vaginal wall are prolapsed. While we can very easily lift up, and even retain in position, the backward displaced uterus and appendages by Alexander's operation, it stands to reason that a permanency of the result will be more assured by restoring to their normal conditions the inferior supports of the uterus and vagina; and often also a laceration of the cervix exists at the same time which maintains a chronic enlargement of the uterus. I therefore always combine with the Alexander's the restoration of these various organs, so far as possible, to their normal conditions. Thus if there is a laceration of the cervix, a retroflexion with descensus, a lacerated perineum, a cystocele, and a rectocele, I proceed in the following order: 1st, trachelorrhaphy; 2d, Alexander's; 3d, cystocele, Stoltz's operation (if still necessary after the uterus is replaced); 4th, rectocele with perineorrhaphy (Hegar's operation); or, if there be no rectocele but simply a lacerated perineum, I restore the perineum by the flap-splitting method. I have frequently been able to perform this series of operations in less than an hour and a half, and I have yet to regret having done so, both as regards the immediate result on the patient (long duration of anæsthesia, loss of blood) and permanent results.

Fourth. After the technique and indications of the operation naturally comes the question as to its results, that is to say, does the uterus which has been successfully placed in its normal position by the shortening and attachment of the round ligaments to the inguinal canal remain in that

position for the rest of the woman's life, in spite of the daily influences which tend to again displace it, and particularly in spite of the occurrence of pregnancy and parturition? This question would be an exceedingly easy one to answer if we were able to retain all, or even the majority, of our patients under observation. Manifestly this cannot be the case, since even in private practice comparatively few operative cases again present themselves, especially not if they are doing well. It is usually only those in whom the condition for which they were treated returns from whom we hear in an unpleasant sense, either personally or through friends. Comparatively few show their sense of gratitude by presenting themselves for the purpose of giving us the satisfaction of seeing how well we have succeeded; hence I can only refer to a certain number of my sixty-five cases of this operation in support of my statement that the operation has been a permanent success. Still the old saying, that "straws show which way the wind blows," may possibly be applied to this operation. I can but say that of all those women upon whom I have operated during the last ten years for shortening the round ligaments, but two have come under my personal observation in whom the operation eventually proved a failure. In one of these the operation was performed for prolapsus uteri, the uterus being very large and heavy, a case in which I now consider the operation contraindicated. In the second, the ligaments had been torn during their attempted extraction, and primarily the operation must be considered to have been at least a partial failure. I must add to these two cases three others in which, in my earlier days, I failed to find the ligaments; here the want of success must not be attributed to the operation, but to the operator. As regards the permanency of the success, I have had the opportunity to examine probably about one half of my cases for a period varying from six months to as many years after the operation, and in none of them, I can honestly say, have

I found the uterus to have deviated one iota from the position in which I put it. At the time of my last article, six years ago, I had not had the opportunity to see any of my cases undergo the crucial test of pregnancy and parturition; but since then not only have Drs. Coe and Boldt reported to me, if I remember right, each two cases, and Dr. Henry S. Stark one instance of that occurrence which had come under their observation, in which the uterus retained after confinement the normal anteverted position; but I myself have seen four such, in one of which I saw the patient in consultation for puerperal septicæmia in her fifth confinement, the other also for puerperal septicæmia in her first confinement, six years after the operation, and in all these cases the position of the uterus was absolutely normal. I do not suppose that it is necessary to say that the septicæmia in these cases had nothing to do with the Alexander's operation, but was merely accidental. Some of my operations performed during the last six months are of course too recent to enable me to draw conclusions as to their permanency. I would say that during the past eighteen months I performed twenty-one Alexander's operations, every one successfully.

In conclusion, while I do not undervalue the difficulties of the operation, and the uncertainties which will always be attached to it, I still must assert my unqualified belief in it in properly selected cases in preference to the other dangerous, and, as regards permanent results, no more certain, operations of ventral fixation and intraperitoneal shortening of the ligaments.

DISCUSSION.

DR. H. T. HANKS.—I am not perhaps the proper person to discuss the paper we have listened to this evening, since I am not such an earnest advocate of Alexander's operation as is Dr. Mundé. I have had some experience with the operation myself, however, and have seen it performed many times at the Woman's Hospital by the late

Dr. Lee and other members of the staff. I have also seen the operation performed on the cadaver in the dead-house of the Post-Graduate school often. Taking my experience during the past seven years into consideration, I have come to think less and less of Alexander's operation as *the* operation for retroversion of the uterus. I do not mean to imply by this that Drs. Mundé, Boldt, Edebohls, Polk, and others whom I might mention, have not performed the operation successfully. The fact still remains, however, as Dr. Mundé said, that it is often a difficult operation, and unless the surgeon has had many cases he will not be able to promise a cure. We have, perhaps, from thirty to forty good gynecologists in this city, and yet out of that number, I would not trust more than five or six to perform Alexander's operation on any dear relative or friend of mine. Those who have done it often can and may do it well. Many of us, particularly those who have been under the teaching of Emmet, and Thomas, and Bozeman, were taught to treat these cases by means of the tampon for a month or six weeks, and then use a pessary. We practised this method successfully long before Alexander revived the operation which now bears his name. I have seen Alexander himself do the operation, and it took him at least five minutes—and it seemed twenty—to find the first ligament, and then not until he had cut down and found the opposite ligament; I made up my mind then, and this opinion has often been confirmed since, that it is often a difficult matter to find the ligament. I sincerely believe that I have cured at least one half of the cases of retroverted uterus that have come under my care by means of the tampon for four to eight weeks, and later the pessary. By this I mean symptomatically and anatomically cured. If, previous to an operation in these cases, we find that the ligament has become stretched two inches or more, there is no reason why it should not become stretched again after the operation, and then you have the same condition of retroversion.

Alexander's operation is not only a difficult one to perform by the average gynecologist, but there is always a possibility of some accident following it. There may be a hernia at the site of either wound, or we may have an anatomical cure and not a symptomatic one, or a keloid may appear in the cicatrix. I believe that in most of the

cases which a tampon and pessary will not cure, parimetritis exists, and a cœliotomy is justifiable. In such cases I always perform ventral fixation. And my results are certainly as satisfactory as my neighbor's results who would do an Alexander's operation in similar cases.

DR. E. H. GRANDIN.—Perhaps, sir, after I have had more time to consider the paper which has been read this evening, I shall be more in favor of Alexander's operation than I am now, or have been in the past. I am inclined to look on the field of this operation as exceedingly limited. I am satisfied that in cases where any degree of descent of the uterus is associated with the backward displacement, Alexander's operation will prove a failure so far as relieving the patient's symptoms is concerned. It is only applicable to cases of simple retroversion of the uterus, cases which rarely give rise to symptoms, and if they do, they can be relieved by the pessary if the uterus be not adherent, in which event Alexander's operation is not indicated. Furthermore, in my own hands, the operation has not proved such a simple matter as it has to the reader of the paper. I have met with great difficulty in finding the ligaments, although in one instance I had directing me an expert anatomist, who had done the operation over ninety times on the cadaver. His experience showed that it was far easier to find them on the dead than on the living. I have successfully performed ventral fixation in two thirds less time than it took me to find the ligaments. In cases where the uterus has been displaced backwards for a long time, the chances are that the ligaments are more or less atrophied and break very readily, as they have in my hands. Again, if I understand the reason why the Almighty placed the round ligaments in the belly of a woman, Alexander's operation does not answer the purpose for which it is undertaken. In performing the operation, we are told, in order to secure a permanent result, the ligaments should be pulled out three or four inches; by doing that you anchor the uterus against the pubes, and no woman's uterus belongs there. In such an abnormal position, it is certain to give rise to pain. It prevents the bladder from becoming distended; the organ cannot thoroughly empty itself and the woman develops a cystitis and cystocele. Such was the result in one case under my observation.

During the past year, I have undertaken Alexander's operation in a number of instances, and I believe I failed to find the ligaments in as many cases as those in which I succeeded, although I closely followed the directions laid down in the books. My opinion is that simple retroversion of the uterus, uncomplicated by adhesions, which cannot be relieved by the pessary, will not be relieved by Alexander's operation, for the simple reason that associated with the retroversion there is descent, and to this the patient's symptoms are due. If the woman demands relief and I cannot secure it for her with the pessary, I do a ventral fixation. Sepsis may occur here, as it also can with Alexander's operation. But sepsis is due in either case to faulty technique. The fact that Alexander's operation, after having been on trial for ten years, is not countenanced by ten expert gynecologists in the United States emphasizes its failure.

DR. H. J. BOLDT.—I confess that I am more on the side of Dr. Mundé than are my friends who have just spoken. The more I do Alexander's operation, the more I am in favor of it. I fully coincide with the statement made by one of the speakers that a simple retroversion rarely gives rise to symptoms. If it does, they are mechanical symptoms, and those due to coexisting perimetritis. This should be relieved by tamponing. Dr. Mundé, in his paper, has given us the indications in which Alexander's operation is called for. There are a number of cases of retroflexion and retroversion which undoubtedly can be relieved by wearing a pessary, but we must bear in mind that the constant use of the pessary is anything but pleasant to the patient, and she may desire to get rid of it.

As regards finding the round ligament, I have performed this operation many times, and have never failed to find it. In one instance the ligament broke readily, due to degenerative changes in its structure. In every other case I have been able to get a complete and absolute cure by means of this operation, without a single failure excepting in my first case, and in that instance it was the fault of the operator. The danger attending the operation is practically *nil*. I think the cases in which ventral fixation is indicated are entirely different from those which call for

Alexander's operation. Sepsis may occur with either operation, due to the manipulation of the operator. If the operation is done properly, the dangers are trifling and the results permanent. I have seen a number of patients upon whom the operation was performed who afterwards became pregnant and went to term, and the uterus remained physiologically anteposed. As regards the occurrence of hernia after the operation, I have seen one instance of this in my own cases, and two instances in patients operated on by others. Whether the occurrence of the inguinal hernia in these cases was due to the respective technique or not, I do not know. I certainly doubt it.

DR. MUNDÉ (closing the discussion).—It is very curious that the experience of one man with a particular operation should be entirely different from that of another. I am fully in accord with the statements made by Dr. Boldt. I believe his experience with the operation has been extensive enough to warrant him to draw conclusions regarding it. *Experientia docet.* I agree with Dr. Boldt that the failure to find the ligaments is the fault of the operator, and those who do not find them either do not know how or else they are careless. On several occasions I have failed to find them, and in all such instances the fault was my own. Unless we make the dissection very carefully, we are likely to miss the terminal fibres of the ligaments. Or, if we go too deep, we are apt to tear off the ligaments. You must either find them within two minutes after you have opened the external inguinal ring, or else you are likely to hunt half an hour for them. I agree with Dr. Hanks that the operation is a difficult one, but if the operator possesses a good knowledge of the anatomy of the parts, he ought to be able to locate the ligaments very readily.

As regards the curability of these cases by means of the tampon or pessary, I wish Dr. Hanks would teach me how to cure old backward displacements of the uterus by such methods. Thirteen years ago I read a paper before the International Medical Congress in London, in which I stated that I had cured very few backward displacements of the uterus with pessaries, and I am willing to confess the same to-day. I would like to ask if Dr. Hanks watched his cases afterwards, and knows how long his cures lasted.

Dr. Grandin referred to sepsis as one of the dangers of the operation. I can easily prove that there was no such danger in my cases. Occasionally, there was slight suppuration in the canal, but nothing serious. As regards a comparison between Alexander's operation and that of ventral fixation, there is no comparison. The man who opens the abdominal cavity of his patient and sews the uterus up takes the patient's life in his hands, and he has no business to do it in these cases. Ordinary backward displacements usually do not produce marked symptoms. The only symptoms may be sterility or a catarrhal discharge. Those symptoms will not warrant opening the abdominal cavity. No matter how skilful the surgeon is, no matter how many hundreds of times he has performed laparotomy, he will occasionally lose one of his patients from that operation. When I have opened the abdominal cavity for the correction of a retroverted uterus, I did it because the appendages were diseased and adherent. To sew the fundus uteri to the anterior abdominal wall for simple retroversion or retroflexion, I consider absolutely unjustifiable. It is simply replacing one abnormal position by another.

Stated Meeting, October 4th, 1894.

CLIMATE AND HEALTH.

By Dr. CHARLES FAYETTE TAYLOR.

In the divisions of land and water, the situations of the continents, the seas, and the islands in the seas; the mountain ranges and the rivers which have their sources in them; the elevations and depressions of the more even surfaces, together with the procession of the seasons and the earth's diurnal revolutions, we have some of the conditions for a great variety of climates. Proceeding from the equator toward the poles, or moving along the surface of the earth in any direction, man, who seems to be the toughest animal on the face of the earth, can so adjust himself to varying climatic conditions as to exist in fairly good health almost anywhere, from the steaming equatorial jungles to the regions of perpetual ice and snow, as well as in intermediate locations where often heat and cold vary from one extreme to the other in rapid succession. And yet men live and thrive in nearly all lands and under the most diverse conditions, and with intelligent self-adjustment to their environment they may live well, and live out their allotted times as a general rule. While the human race is exceedingly flexible, and can adapt itself rapidly to very diverse conditions, such adaptations, be they rapid or relatively slow, are not accomplished without an expenditure of energy to correspond with the functional modifications thus brought about. We call the process acclimatization, and the person, after subjection to the process, we say is acclimatized. That is to say, the functional activities of

such a person have become adjusted to his environment; his functions have learned to harmonize with the temperature, food, humidity, and other influences affecting him. The effect on the individual varies according to his susceptibility and the degree and intensity of the factors acting on him. In some cases sluggish functional activities are energized with a tonic effect. But when that is the case it does not follow that the new climate is necessarily intrinsically better than the one from which he came. In other cases the effect of climate change often proves atonic, depressing, and injurious; but a bad effect on an individual does not prove that the climate is necessarily worse than the one to which he was formerly accustomed. It may happen that influences, good in themselves, may be injudiciously employed; as if a person requiring a cooler climate, such as that of the coast of Maine, for instance, should try Labrador, and it should be more than he could bear; or, if one needed a milder climate and should find Para too depressing. Even the relatively slight difference between the European and American sides of the Atlantic often proves serious to the immigrant during the process of acclimatization, and many succumb, though it is probably true that the majority of immigrants find themselves invigorated in their new conditions of life on this side of the Atlantic.

My object is to call your attention strongly to the well-known fact that change of climate and its attendant circumstances, even when not of any extreme character or degree, does produce an impression more or less profound on the vital processes, and that the nature, degree, and general therapeutic or pathological character of these influences should be more carefully studied than they have thus far been studied, so that when consulted by our patients we may have some definite advice to give in regard to locations best suited to the inquirer's special needs; or, if we can not do so much, we ought at least to

be able to give our patients some very positive ideas as to the kind of climate to seek, and especially what to avoid. For instance, California is seven hundred and seventy miles long. It embraces, according to Dr. Remondoin, at least seven climates, distinctly different from each other, and all very different from the climatic conditions existing on this side of our continent. What is the sense in telling a patient with a hole at the top of his lung to "go to California" without instructing him in regard to the location to which he should go, or at least what kind of climate he should look for? Without some specific information, such a patient is likely to drop into a place better calculated to shorten than to prolong his days. To be sure, all the climates of California are characterized by a dryness exceeding what is known in the East, and this fact gives some relative advantages. But unquestionably, the air may be too dry in certain localities for certain cases. Is it not too much to expect a patient to find out what the doctor, who sends him away from home and friends, himself does not know? Besides, there are many constituent elements which enter in to make up what we call "climate." The first of these to be mentioned is usually the thermometrical readings, and the "mean" temperature is generally quoted as proof positive of superiority when it varies a few degrees one way or the other from that of another locality with which it is compared. Now, the truth is, that to know the mean temperature of a place, and to know only that, is to know very little about its climate. The physiological effects of a climate must necessarily include the degree of humidity, the force and direction of the prevailing winds, the sunshine and cloudiness; the fogs and their characteristics—whether thin or dense, high or low, whether coming down from the mountains or rolling in from the sea; besides other unmeasurable influences not seen though felt: all these and more must be appreciated in order to give the single factor of relative temperature any positive

quality whatever. For instance, the mean temperature of the seven hundred and seventy miles along and near the coast of California varies but a few degrees, though the extremes vary much. But the physiological effects of the climates of different areas vary greatly. There are stiff northwest winds from off the Pacific, carrying a thin, swift-moving fog that chills an invalid to the bone, during July and August in San Francisco. To correspond to the sensations, the thermometer there lies like a cheap watch, and should be twenty degrees lower. A few miles back from the coast, with less wind and little fog, one's bodily comfort is perfect, and life is worth living, though the unlucky thermometer persists in recording nearly the same average as when you had been shivering on the coast. I conclude that the physiological influence of a given temperature below a certain degree, say below sixty, with the wind ten or fifteen miles an hour, is equal to at least ten or fifteen degrees lower in scale. On the other hand, a thermometrical showing of 90° and over is not uncomfortable if there is a gentle breeze and little humidity, but with a strong wind becomes a sirocco, when prostrations are numerous, and, if long continued, many aged and feeble die under its influence. In one of the interior valleys of California I have seen the thermometer indicate 100° to 110° F. for days and weeks together, and no one complained of the heat as excessive, while all labor of man and beast went on as usual, and prostrations were unknown. I refer to the temperature in the shade. In the sun, where men work, it must be ten or fifteen degrees higher. In New York, when summer heat approaches 90°, we expect many prostrations and some deaths. I am not trying to show that 110° of heat in California with no prostrations is a better climate than New York at 90° and many prostrations, but to illustrate the principle that we must know much more about a climate than what the thermometer can tell us before we know very much about it.

I kept a record of the temperature in Martinique, one of the Windward islands lying in 14° north latitude, and it never went above 86° F., nor so high but two or three times during a residence of three seasons, and once so late as the first of July. But no sensible person would dare to expose himself to the midday sun with the same impunity that he could in this latitude, and a corresponding temperature. The ever-present humidity, bordering on saturation, in the tropics is an important modifying element to be taken into account.

Again, temperature may depend on latitude or on altitude; but it is not a matter of small moment which the cause may be. Sixty degrees of heat at the level of the sea and on the seashore are very unlike, in physiological effect, to 60° in the dry and rarefied air of an elevated inland situation. There is no doubt that considerable moisture in the air favors the growth of minute organisms, and decomposition of matter takes place rapidly under the influence of heat and moisture. On the other hand, a dry air retards decomposition, and, if sufficiently dry, prevents it entirely, no matter how hot it may be. The Sacramento valley is very hot in the summer, but it is also dry, so that friends of mine would kill a beef, and elevate the carcass by means of rope and pulley to the top of a tall pole, let it down from time to time to cut from it, and it would keep perfectly sweet until it was all eaten up. A good illustration of conditions retarding or favoring the growth of minute organisms may be seen during the orange harvest in portions of California. The altitude of Redlands, California, averages about fifteen hundred feet above sea level. Fogs seldom reach there, the sun shines clear more than three hundred days of the year, and there is not a speck of mildew on any fruit. But go forty miles nearer the sea and seven or eight hundred feet down nearer the sea level, and at every station you will see many people washing oranges. More fogs, denser air,

less sunshine, more humidity, favor fungous growths. On the high table-lands of the central portions of the continent, at an altitude of six or eight thousand feet, as in Wyoming, where a friend lives, milk does not "sour" or change under a week or ten days, and the carcasses of dead cattle, of which there are many, give no offensive stench, but slowly dry up and waste away, showing that comparatively few organic germs exist there, and that the conditions for their rapid propagation are unfavorable: From the facts just stated—and they are representative facts—it would be too hasty to conclude that the higher and drier locality is essentially more healthy than the lower and moister locality, even for consumptives, until we have mastered and estimated the quality and energy of the other meteorological influences.

There is another fact which has come under my personal observation, which must be taken into consideration. It is that life in those elevated non-microbian regions is not without its drawbacks. Whether it is due to the increased action of the heart in the rarefied atmosphere, the constant hammering of the nerves by the winds and the fierce sunshine, or all these and other causes, people in those regions have a thin and tired look, and it is found useful and often necessary, especially in cases of women and children, to visit lower, damper, and more germ-laden regions in order to recuperate. It is important that the air we breathe should contain as few disease germs as possible; but it is still more important that we should breathe an air, and live under such climatic conditions, as shall most conduce to such general bodily vigor as will resist the entrance of disease germs into the organism, or destroy them if an entrance is once effected. It is quite conceivable that a dry atmosphere containing few microbes may be too dry for an irritable mucous membrane, and set up catarrhs which may furnish nesting places for disease germs; while a moister, softer air, though holding

many more microbian elements, may be more advantageous, at least in certain cases.

In these latter days, in the wonderful strides which have been made and are constantly being made in bacteriology, perhaps we are in some danger of losing sight of meteorology in its relations to health and disease. It seems to me that climatology has heretofore, to a large extent, resolved itself into a search for some place where consumptives cannot die. There is no such place. There is no place where the ever-present bacillus may not get in its deadly work. The chief question in climatology in its relation to health should be, "In what climate, or by what changes and influences of different climates, can we be best invigorated for good existence in the location where we are obliged to live the greater portion of our lives?" Many other causes besides tuberculosis men die of. Among civilized people, especially among our pushing Americans, debility, nervous exhaustion in one form or another, from over-activity of brain or body, render multitudes asthenic and vulnerable to the invasion of disease. We say that such cases need to be "toned up." This is undoubtedly true, but there are many cases in which the first step in "toning up" should properly be to tone them *down*. By that, I mean that it is necessary to diminish the unnecessary expenditure of energy which has become a fixed habit of life.

We all, as a rule, are too prodigal of our resources, and squander vast quantities in excess of what the occasion requires. It is amazing to see people, intelligent about ordinary things, traveling for their health at a rate that suggests that they have been shot out of a gun. Many do, of course, get a limited benefit in the change of subjects of thought, but they often mistake change of feeling, due to excitement, for recuperation. We need to learn how to stop. Instead of rushing across the face of the earth in the delusive hope of finding health on the other

side, we need to learn how to sit down and make ourselves comfortable where we are. A man who had lived to a great age in health and contentment, was asked to give some simple rule of life out of his experience. In reply, he said, "The only rule I can give is, 'Always keep comfortable.'" I feel confident that a well-selected residence in the tropics, from time to time, will prove helpful in acquiring habits of reposefulness.

Tropical heat is not oppressive, as many who have not tried it seem to suppose. It is very different from the same temperature as indicated by the thermometer during a northern summer. One does not fret about the tropical heat as he is apt to do here, but is inclined to keep quiet, lie down and sleep a good deal during the daytime, as well as profoundly all night. Wakefulness is a rarity. The relief from nervous tension and irritability is inexpressibly delightful. The increased action of the skin relieves and gives needed rest to overworked kidneys; the air passages are bathed by a moist, bland, non-irritating warm air; no chilly draughts scourge the nerve centers into activities wasteful of energy; morbid appetites are allayed; digestion is improved, in sympathy with increased skin activity; and the poor invalid begins to feel that, after all, life may be worth living.

It is a delusion, born of constant assertions of the advocates of negro slavery before the war, that white people cannot work in the tropics. The island of Porto Rico was originally settled by Catalonian peasants, and the major part of the farm labor has from the beginning till now—say for approaching four hundred years—been done by white men. True, negro slavery was introduced there, but of a milder type than in the other islands; and the blacks never amounted to much more than one third of the population, and they rapidly mixed with their Spanish co-laborers, beside whom they worked. The facts are still more startling in regard to the Spanish Main. Along the

coasts of Central America, the mahogany cutters, called "Indians," are mostly of mixed negro blood; and along the unhealthy shores of the Magdalena river, or wherever the sugar cane is cultivated, negro slaves were introduced, and their descendants, largely mixed with the Indian race, still remain. Even in Brazil, the negroes and their descendants are confined to a few provinces, and never to exclude white labor; and in numbers the African blood constitutes but a small proportion of the ten or twelve millions in that country—certainly not enough to influence the following statement: From the southern border of the United States through Mexico, the republics of Central America, Colombia, Venezuela, Ecuador, Peru, Bolivia, Chili, Paraguay, Uraguay, the larger part of Brazil, Argentina, down to Patagonia, with the exceptions above mentioned, there is not now, and never has been, any farm labor but white farm labor since the settlement of that vast continent; and the widest portion is directly under the equator. I do not include the Indians in this statement, because when wild they do not work in the sense here meant, and when brought under the influence of the Spanish and Portuguese civilization they immediately mix with, and become essentially one with, their white co-workers. I do not deny that there are pestilential lagoons, which are more pestilential than any similar territory to be found north. But I do not believe that, shunning local conditions which would be bad anywhere, and worse in the tropics, well-selected locations are unhealthy because of tropical heat and moisture, except in certain cases. On the other hand, I believe that almost all elderly people and a large number of over-worked and tired-out persons would find that tropical life costs a largely diminished outlay of energy, with a corresponding husbanding of nervous and metabolic forces. In illustration of the foregoing statement, I give the following facts:

The island of Dominica lies in fifteen degrees north lati-

tude, and contained twenty-nine thousand people in 1885. Dr. Nicholls, the chief medical officer of the island, whom I personally know, made a report to the managers of the Colonial Exhibition held in London in the year 1886, in which he stated that the death-rate of the preceding year was fifteen and a half per thousand of the population—that is to say, the death-rate in this small island, deep in the tropics, was less by ten per thousand than the average for New York city. The people are mostly blacks. Further inquiries revealed the fact that there were, at that time, three hundred and ninety-one white people—men, women, and children—and that there had been two deaths among them during the previous year: one from apoplexy, and one, a nun, died from phthisis, which she had brought from England, she having come to the island in the hope of benefit to her health. In fact, there was not one death among the whites from any disease generally supposed to be especially tropical. The death-rate is higher in some of the other islands, but not higher, according to the best information I could get, than in northern communities from the same classes of diseases. It should also be remembered that many of the West India islands are in a deplorably bad sanitary condition, exposing them to be scourged from time to time by importations of yellow fever, small pox, and like epidemics, when, of course, the death rate is largely increased. The foregoing applies more especially to the Windward islands, which possess some conspicuous advantages over the Greater Antilles, in that they lie in the path of the northeast trade winds, and, being small, the winds sweep over them as over the deck of a ship. There is no alternating sea breeze and land breeze, because there is not sufficient land to be heated by day to form an upward current, and to cool by night and form a downward and outward current; but there is always a gentle movement of the air toward the west, without the intervals of calms which characterize

the Greater Antilles. But any of the West India islands, no doubt, furnish many locations in good sanitary condition, where the intelligent invalid may find bodily and mental repose, and let his muscles relax and take comfort, while his enfeebled skin, long constringed by cold, and debilitated by clothing, is stimulated by genial warmth to doing its long-neglected duty, for a time at least, while the kidneys, heart, and lungs are given a much-needed rest.

It should be understood that I do not think all cases would be benefited by a sojourn in a tropical climate, but I feel assured that for a large number of carefully selected cases, no resource of climate can be so promotive of improved metabolism as a well-selected location, and suitably regulated life during several winter months in the tropics, from time to time, especially if closely followed by a change to a higher latitude or altitude, and cooler locality during the following summer.

Not least among the advantages of a tropical climate for a temporary sojourn in certain cases is the change of food, which is, or at least ought to be, always effected. It would be a very unwise proceeding to subsist upon a diet essentially the same as one is accustomed to at home. In the first place, there are not many of our American stomachs that do not need a rest, and one of the objects to be sought in living in a warm climate is to give the overworked stomach a chance to recuperate; not only because there is no necessity for the same amount and quality of food to be digested, but we can find in the fruits of a country food which is not only very easily digested, but which supplies nearly all the requisites for wholesome nutrition under the changed conditions. The best fruits of the tropics are very perishable—so much so that we never see them in New York. Now, I am not advocating an exclusively fruit diet; but I think when people from the north go to the tropics for "climatic therapeutics" they should

make it a point to eat very sparingly of meats and even farinaceous food, and endeavor to supply Nature's wants by using largely of the fruits of the country—especially those soft, sweet, and perishable fruits which do not last more than a day or two. Thus we have, besides the influences of steady warmth and moisture, the added advantage of a change of diet, which is no small factor in modifying the metabolism which we seek.

I have spoken of the Windward islands as being especially desirable during the three or four months of the so-called "dry season," or from December to May, and of the whole West India islands as furnishing desirable locations for climatic rejuvenation. The West Indies are especially interesting because communication is so easy and constant and relatively cheap; they are practically at our door, and it seems to me that they should be studied more. The Spanish Main also furnishes a great variety of especially desirable locations, which can be used for the same purposes; but in speaking of the question of climate in "therapeutics," my object is not to advocate any particular point, but to illustrate the general subject.

When one has become rested by some months' sojourn in a tropical region, and, as the season advances, goes north instead of sweltering in New York, or other corresponding place, it would be well to go to the seashore or to the mountains, where he would receive another form of tonic to his already partially recuperated energies. In that way we should be using the climate as an essentially "therapeutic means."[1]

The larger number of invalids and tired-out people will continue to go to Europe for their change, and undoubtedly that is the better course for the majority, and, when properly managed, the "therapy of climate" may be suf-

[1] For more detailed information in regard to the West Indian climate, I refer those interested in the subject to several articles in *The Times and Register*, by Dr. William F. Hutchinson, beginning in the number for September 6, 1890.

ficiently realized in that manner, in most cases. I do not include those people who travel for pleasure only, or where change of climate is the secondary object, though in many instances even those persons do reap real advantage from the considerable change in food, air, and the surrounding conditions of life.

There are many advantages to Americans in visiting Europe, not the least of which is the change of interests which new and different objects for contemplation furnish, and that fill the mind without taxing it, to the temporary displacement of the business, political, domestic, or other cares and anxieties which are apt to hold our American mind in a tenacious grip, from sheer force of habit. With three thousand miles of ocean behind us, it is not easy to talk "shop" with the neighbor at our elbow, during the ten minutes some people devote to their lunch or dinner, and we are almost obliged, by prevailing custom, to take a reasonable time at meals, and be quiet about it. I believe that the climate of Europe is no better than ours, and in some respects not so good. I am told that life insurance statistics—the most reliable of all—show that the life expectation is somewhat longer among American risks than in European; and there seems to be no evidence among athletes or race-horses that bodily vigor is not equal here, to say the least, to anything across the water. But there is a difference in meteorological conditions, and this difference may be very effectually used to invigorate and improve the metabolism, in a large number of cases, if we keep this object steadily in mind, and manage toward the accomplishment of this end. There is not only the mental relaxation not possible here in the midst of ordinary pursuits, but the change of climatic conditions, though not so great as a change to tropical lands, is still considerable—quite enough, when properly utilized, in connection with mental and bodily rest, change of food and cooking, change of many habits, and the gentle, but

quite positive, mental tonic of new scenes and new interests. With some important exceptions to be presently noticed, I do not think it makes very much difference where our American tired-out or half invalids go, provided they actually get rest, and always keep comfortable. Of course, I do not include those thousands who are always on the rush, "doing" Europe.

Among other influences there is no doubt, in my mind, of the great therapeutic value in many cases of well-regulated courses of mineral waters, when the cases and the waters are carefully selected and as carefully directed to the peculiarities of each case. Nothing could be more reprehensible, from the therapeutic point of view, than for an American family to turn itself loose in Carlsbad, for instance, and drink haphazard of these powerful waters—powerful for harm as well as for good—without the advice of a competent physician, experienced in their use and effects, as I have been told our brethren sometimes do. My experience with physicians at some half dozen European spas has been very satisfactory, and leads me to believe that the local doctors are generally capable, honest men, and that their advice ought to be generally sought and followed with confidence in the use of the waters. But we, on this side, ought to be able to give clear advice, if not as to the particular mineral spring, at least as to the general character of the waters to be sought; and especially it is always important to urge our countrymen not to overdo the matter. I am speaking more especially of that vast horde of tired-out health-seekers who annually cross the Atlantic, and for whom there can be no better therapy than judicious change of climate, including mineral waters for a certain number, as an added and potent alterative. Taking Carlsbad as perhaps the representative spa of the Continent, I am of the opinion that there are few middle-aged or elderly persons who are not decidedly the better for having their capillaries physicked,

and their emunctories cleaned out once in a while by a course of Carlsbad water sufficient for that purpose; and it is astonishing what a small quantity of the water will sometimes do it to the extent here contemplated.

There is some danger to the novice in going into semi-tropical regions, in being unacquainted with, and unprepared for, the degree of apparent cold which he is likely to find to his great surprise. And when he looks at the thermometer he is further surprised to see it so high, while his feelings indicate a much lower temperature. He is still more astonished to notice that the natives do not mind the cold that makes the novice shiver. The fact is, that without his accustomed fire and house-warming facilities, and subjected to air currents, the practical temperature in its physiological effects is much lower than the thermometer registers. In Spain, Italy, and in general along the Mediterranean shore, they have a semi-tropical climate during eight or nine months of the year, during which time the native inhabitants hold their calorifacient function in reserve, and when they reach their short and moderately cold season they have no difficulty in drawing sufficiently on their reserve heat-making power. The man from the north has no such reserve, and what he has, the temperature is not sufficiently stimulating to call into full activity. He has used up his caloric in the greater cold of the north. People from the extreme south enjoy their first northern winter. I met, in Teneriffe, an intelligent captain of a whaling ship who had several times fished in the Bering sea. He said it is customary for whalers to make up for loss of men from desertions, by taking on South Sea islanders. He said they bear the cold and hardships of the north as well as New Bedford whalemen, and in proof related the following incident: One morning, when far north, he noticed on coming on deck, one of his South Sea islanders entirely naked, taking a bath. There was a strong wind blowing, and it was so cold that the water he

dashed over him froze as it struck the deck. The man seemed to enjoy it, though he had never seen frozen water or snow before. There are good reasons why people of the north with impaired stamina should not expect to bear exposure so well as natives of semi-tropical regions, and should make themselves, in regard to temperature, more comfortable than would be sufficient for the natives.

Northern people should be particularly careful in going to a climate with a temperature too low for comfort without a fire and too high for comfort with a fire. Even the increased sunshine is not sufficiently constant, and all rooms do not face the south. No matter what natives may say, Americans ought always to have the means for heating when occasion requires, and a southern aspect to their rooms everywhere in southern Europe, if they are at all sensitive to cold, irrespective of the thermometer, or else travel north till they come to fireplaces, stoves, ovens, or other means for artificial warmth. Inquiry into the sanitation not only of residences but of the towns should never be neglected. I suppose the climate of the southern and southeastern coast of Spain is perhaps the most genial on the Mediterranean, and equally the most dangerous for Americans to abide in on account of the lack of proper drainage and other attention to sanitation. But the same may be said of much of the coast except on portions of the Riviera, where in certain places much improvement has been and is being effected in that respect.

There are generally good reasons for many of the customs and habits of the natives of any region, and there will be found advantages in adopting many of their ideas and methods so far as practicable. Along the Riviera people flock indoors with the going down of the sun; and there is good reason for it. At Nice, I have seen the thermometer register a fall of 25° F. within an hour, as the sun neared the horizon. Such sudden cooling might be dangerous to an American dyspeptic with his limited power of reaction.

Going indoors reduces this difference of temperature. Right across the Gulf of Lyons, in Barcelona, Spain, in nearly the same latitude and about the same mean temperature, the habit of the people is to be out of doors in the evening, promenading, visiting theatres and cafés, and the ladies doing their shopping till midnight and after. They find evening the best time for many purposes, because there is very little change between the temperatures day and night. My thermometer was hung in an alley which the sun never reached, and all I could make it do was to record the extreme difference of two degrees between six o'clock in the morning and two o'clock in the afternoon during a week. I was still a thermometer dupe at that time. I have since broken my thermometers, and they will never endanger my sanity any more.

But it is not always convenient or even possible for one needing the therapeutic advantages of change of climate to go to Europe, nor is such a change necessary or even desirable in many cases. There is a great deal of as good climate as the world affords in our own country; and almost any change from low to high temperature, from damp to dry, from low to high altitudes, from seashore to mountains, from regions of high cultivation to the balsamic air of primeval forests or the reverse, can be had without the fatigue and expense of long sea voyages and wide stretches of turbulent sea between the traveler and anxious friends at home. The "sunny south" offers much that is admirable both in quality and variety of climate suited to various conditions. The main idea should not be the search for the perfect climate which does nowhere exist, but the question should be, "What change is indicated for the case in hand?" The question, to be properly answered, necessarily includes a knowledge of the region in which the individual has been living. Shall an inhabitant of Virginia go south or north for the winter? Or, had he better go west, or northwest or southwest? Shall a New Yorker

go to Florida, and, if to Florida, shall it be into the tonic Atlantic breezes of the eastern shore, or the milder and softer air of the Gulf coast? Does this person's condition and meteorological surroundings indicate a change to the rarefied air of Colorado or to the denser atmosphere of Tennessee, Michigan, or Minnesota? Ought the change to be to the moist breezes and frequent rains of Washington in the northwest or to the constant sunshine and more even temperature of southern California in the semi-tropical southwest? If the Pacific slope seems indicated, shall it be in the coast cool winds or the warm and calm interior valley? Or, had we not better make a new climate of our own? The bosom of great Ocean furnishes a variety of climates all its own. In former days, a sea voyage was much resorted to for chronic invalids, and with decided advantages in many cases. A life at sea, if at all prolonged, has the disadvantage of leaving too many comforts behind to be recommended for any but the young and comparatively robust. It is said that three hundred miles from land the air is free from living germs. Many persons have returned from long voyages in health entirely restored.

But we need not go to sea, or go abroad, or even to the south, or to the wide west. We can make a climate of our own if we properly work for that end. It is not even necessary to rattle over ten miles of pavement in order to get a change of air. We have it right there on the veranda; we can have it fresh from the outside grand air, by opening the windows and opening them wide till all the stale air in the room is blown out and all the room is filled with ozone. We can change our food at home if we like. I know a man who almost rejuvenated himself by living on little else than fish, oysters, and clam juice for three months. We can regulate the temperature and take a sun bath whenever the sun shines. And we can stop fretting if we are sufficiently determined to do so. Actions

are but the evidence of a predetermination. Why not determine to change our climate, when there is benefit to be derived from such change?

It has always been the unsolved puzzle of my professional life that so many people insist on reaching out to a distance for much that can be had better right at hand if they would but open their hand and take it. This is especially so in regard to climate and changes of climate. Notwithstanding positive directions to the contrary, many a child in pain with joint disease has been taken out miles over rough pavements "for the air," while every jolt was agony, when the same air could be had in his bedroom, with pleasure and safety, by bundling him up and opening the windows and keeping them open. It is against the law to live in cellars, but we make cellars of our rooms by keeping them filled with impure air. I do not inveigh, as it is the fashion to do, against the temperature at which our American houses are kept; a higher temperature is a necessity of our climate; but some one has yet to secure a fortune and the blessings of mankind by devising a system which will keep our houses always filled with living, moving, fresh air, and that will oblige everybody to attend to this matter as he ought.

And here I wish to enter an earnest protest against the practice of sending patients, often far gone with consumption or other wasting disease, away from friends and the comforts of home, without knowledge of what would be best for them, in a fruitless search for health, when, in their enfeebled state, better conditions could be instituted at home, where at least they could die in peace. Some, not too far gone, do recover, it is fortunately true, but many lie buried there, and more are sent east in long boxes. On my last trip east, a young girl sat in front of me, whose mother's body accompanied her, and opposite me sat a gentleman and his wife whose daughter's body was also in the baggage-car. In neither of these instances had the

invalids been more than a short time west. Too many such things are happening for the credit of our profession. Send patients in time, with a definite intention in the change of climate sought, or do not send them at all.

My object has been to call attention to the many and often difficult questions involved in the therapeutics of climate in its wide and varied significance. Probably no one now lives who is capable of answering all the questions relating to "therapeutics of climate," but they will be answered some day and correctly answered; and when answered it will be found, I believe, as a general thing, that the best climate for consumptives is also the best for other persons in like general physical conditions. Twenty years after our profession more fully realizes the immense value of "climate in therapeutics"—and hundreds of capable men have been studying the subject from that point alone, and valuable material has been created to draw upon—a climato-therapy may be formulated which will give the divine art of healing a new uplifting, not less glorious than that which in our day has attended the labors of Pasteur, Koch, Lister, and others whose immortal services have so enriched the world.

DISCUSSION.

DR. S. BARUCH.—I was very much interested in listening to Dr. Taylor's paper. It is refreshing and instructive to hear the deductions of a man who has visited many countries and lived in many climates, and who does not laud any particular climate in which he is personally interested. It has been my aim during many years to reduce the practice of medicine to simple principles, because it is only by these common-sense principles that we can combat disease successfully, and this rule I have endeavored to carry out in regard to climatic therapeutics. I find myself in accord with the author. When we meet with an intractable disease, like phthisis, we are painfully reminded of the futility of drugs, and a change of climate occurs to us as an escape from the dilemma. Here great confusion confronts us.

The ideas prevailing to-day regarding climatic therapeutics remind me of those held regarding aseptic surgery some years ago. Then the surgeon was not content until he had saturated the air of the operating room with a carbolized spray, and douched the wound with strong antiseptics; unfortunately, he was not so particular about the condition of his hands and instruments as he is to-day. Since then, observation has shown that the very essence of aseptic surgery may be summarized by the two words, absolute cleanliness. When I was a student, we were taught that all phthisical patients should be sent to a southern climate; the gentlemen who so taught me would have been horrified if the proposition had been made to send such patients to the Adirondacks, to Colorado, etc., and yet we know that just as many consumptives recover in these seemingly inhospitable climates as in the south.

The point I desire to bring out is, that the principles of climatic therapeutics should be simplified, and I believe that before long it will be recognized that, given pure air, a well-drained soil, and plenty of sunshine, we have nearly all the elements which constitute a good climate for most of those patients who suffer from diseases for which they are now exiled from home. The most important point, to my mind, in Dr. Taylor's paper, is the reference he makes to the climatic advantages which are obtainable at our own homes, and our failure to utilize those advantages. Too often a patient is sent to some far-off clime when the same, or even better, advantages could be obtained for him at home. By methodical attention to the ventilation of the rooms; by so arranging the patient that he will not be exposed to draughts; by properly drying or moistening the air, if necessary; by regulating the diet; by arranging the hours for rest and exercise; by proper baths; by all these hygienic measures, which the medical profession has long known, but which are not so systematically used as are drugs, we can often do more for the patient at home than by sending him away. The methodical, hygienic treatment of phthisis, for instance, will often accomplish wonders. In Dettweiler's sanitarium, at Falkenstein, where it is so cold that the ground is covered with snow all winter, the good results have been mainly attained through hygienic means. All the actions of these patients

are regulated by the physician in charge; he tells them when to go to bed, and when to get up; he feeds them properly, and even teaches them how often to cough. What is the result of this treatment? Thirty-seven per cent. of all cases are discharged cured. We have tried to carry out this same method, in a less systematic way, at the Montefiore Home, where a sun room was built and set apart for the purpose of providing an artificial climate. Here the patients can be so arranged that they are exposed to the fresh air, and at the same time the temperature of the room is kept at 65° F. Our results with this method of treatment have been very favorable. In my private practice, I have carried out the same plan of treatment successfully. I am sure that if we give our phthisical patients plenty of fresh air, and all the other advantages to which I have referred, we shall not so often find it necessary to send them to other climates. I have so treated a case from Minnesota successfully in midwinter, and have one now from Florida. At the same time, we must not lose sight of the fact that if this systematic method of treatment could be carried out in a more favorable climate than our own, the results would be still better.

DR. R. C. M. PAGE.—Dr. Taylor, in his paper, has called our attention to a very important subject, and his conclusions are drawn from extended personal observation and experience. In considering the desirability of a change of residence in any given case, it seems to me that three important points should be kept in mind: (1) The nature of the climate in its widest sense, including elevation and the extremes of temperature rather than the mean temperature, which is always misleading. Also the nature of the soil; whether it is well drained or not. (2) The accessibility of the proposed locality, and its hotel accommodations. (3) Last, but not least, the condition of the patient. I believe in the majority of cases it is the change of residence rather than the climate which produces the beneficial results. There are exceptions, however.

For instance, damp localities, with cold, chilly nights, and marked and sudden variations of temperature are certainly contraindicated in most cases of pulmonary trouble, notably phthisis. A cold climate with dry air and slight changes of temperature is preferable to a warm climate

where these conditions do not prevail. High altitudes, although, as experience has shown, not particularly contra-indicated in cases of pulmonary disease with hæmoptysis, are certainly not favorable for patients with cardiac trouble, or with nervous disease, especially when attended with insomnia. The nature of the soil must also be borne in mind. Other things being equal, I would recommend a sandy soil, where the water could easily escape, so that the accustomed exercise of the patient might not be interfered with.

As regards the practice of sending patients to one of the various well-known watering-places or mineral springs, I think there is no doubt that many such persons injure rather than benefit their health by the injudicious use or rather the gross misuse of the waters, baths, etc., during their sojourn. For that reason, they should be placed under the care of a competent physician there, rather than follow the advice received from their friends or from one of the hotel servants, as is too often the case.

After we have decided upon a suitable place, the question comes up, How is the patient to get there? There is no doubt that a long, unbroken journey, attended with much fatigue and discomfort, will often counterbalance any good that may be derived from the sojourn. European physicians, in order to obviate this, request their patients to break the journey, especially when traveling in high altitudes.

When it comes to the condition of the patient himself, I should be opposed to the removal of persons in whom the disease has progressed so far that they are incapable of helping themselves or of taking a certain amount of exercise. It is better that such patients should remain at home, surrounded by their friends and the comforts it affords, rather than to subject themselves to the annoyances of travel, which will certainly injure rather than improve their condition.

DR. A. ROSE.—The sanitarium for consumptives of Dr. Dettweiler at Falkenstein, to which Dr. Baruch referred, and in which such good results have been obtained, is filled mostly with patients of the working class with limited or no means. The institution is sustained by contributions from large cities, especially Frankfort-on-the-Main. The

mentioning of this sanitarium gives me the opportunity to suggest the establishment of a public sanitarium for tuberculous patients of the working class, large enough to accommodate applicants from all parts of our country, to give patients the benefit of climatic treatment combined with the most advanced methods of therapeutics in general. This is to be in contradistinction to "Houses of Rest" or "Homes for Consumptives," where cure is a secondary consideration, and a fatal termination taken for granted. The acknowledged climatic factors for the cure of tubercular consumption are dryness, elevation, and equability of temperature. This calls for a location at some considerable distance from the ocean—the great source of atmospheric moisture; for an elevation not less than 1,500 and not more than 5,000 feet above sea level; and for a more or less southern latitude, to insure a reasonable degree of equability of temperature. What is imperatively needed and universally acknowledged by the medical profession as an absolute necessity, is a sanitarium for patients of moderate means, and, if possible, within easy reach of all the large cities of the Atlantic seaboard. Places in mountainous regions, where consumptives are sent by their physician to shift for themselves, are totally inadequate. The medical profession should be interested—the leading local, state, and national societies and all the medical journals. The institution should be made self sustaining, and furnish much-needed occupation to a large class of patients who would otherwise be brooding over their misfortunes. To this end a tract of land should be acquired, sufficiently large to raise vegetables, fruits, poultry, and cattle, which would supply the institution with food of the best kind on the one hand, and furnish light occupation to patients on the other.

DR. E. H. M. SELL.—I wish to express the gratification I felt in listening to Dr. Taylor's paper. Certain points were brought out by him which are well worth consideration. I fully agree with his statement that even where the climate is an ideal one, unless other conditions are also favorable in a general way, we should hesitate before sending our patients there. I have traveled a great deal and can emphasize the importance of this statement. Instead of telling your patient in a general way to visit

Egypt, unless he will live on board a dahabieh on the Nile, put him beforehand into communication with an American or European physician there, who will be able to inform him regarding existing conditions; for while the climate in a certain place may be perfect, the dust, the exhalations caused by irrigation, and other local difficulties may aggravate his bronchial or pulmonary condition, etc. Cases did well at Algiers, Africa; Aguas Calientes, Mexico; Santa Fé, on the Isle of Pines, fifty miles south of Cuba; and in various places in our own country, such as San Antonio and Frio county, Texas; Colorado, during the summer, and Los Angeles and other places in California during the winter, etc., where the climate is excellent for pulmonary patients, but unless they receive proper and timely treatment at the same time, they must stay there, otherwise they will have a relapse when they return home.

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DEFECTIVE VISION IN ITS RELATION TO CRIME.

By FRANK VAN FLEET, M. D.

The subject of crime presents to the student of human nature so many sides that, in its study, he is at a loss to know just which side to attack. It is perhaps natural that each student should begin at the side which, in his judgment, seems the most important. Naturally, the physician begins with the causes, and ends with the cure.

In carefully looking over the ground, we find that we can divide the causes of crime into two groups, namely, the moral and the physical. It is not possible in a paper on the causes of crime from a medical standpoint, to separate the physical and the moral entirely. They are so dependent on each other, and both so important that we find our thoughts constantly drifting backward and forward between the two.

A comprehensive study of crime and the criminal would occupy more time than we have at our disposal to-night, and I have therefore endeavored to limit my remarks to one of the most important physical causes, namely, defective vision.

The proportion of crime and criminals to population is increasing. Writers are not wanting who will tell you that education increases crime. The statement is true; education and enlightenment do not only increase crime but cause it. To make this unqualified assertion, however, is unjust and misleading. These factors do increase

crime, but not necessarily in the possessors themselves. The increase is rather among those who have to compete with the educated. So it might be, and is, said that it is the very lack of education that is the cause. But neither education nor the lack of it is in itself sufficient to cause crime, as I shall show later on.

Crime dates back as far as history extends. Thousands of years before the historical era, long before the days of Moses and the Patriarchs—as long, indeed, as forty thousand years ago—man wandered upon the face of the earth in herds, even as a few years ago the buffalo lived on the Western plains. There were in those days no ties of family; father and son, sister and brother, were no more to each other than so many members of the common flock. Children honored and respected their mothers only so long as the mother was necessary to the offspring's existence. In those days there were no crimes, because there were no laws. The existence was precarious. Every individual was a law unto himself. The strong triumphed over the weak because might made right. The weak and delicate, if there were any, perished. The law of the survival of the fittest here found its most marked exemplification. Flourishing in the warmth and luxury of the summer, freezing and starving during the long and severe winters, this herd of animals of perfect physical development, lived on. Slowly, on through the centuries, this existence continued, every generation probably advancing beyond the preceding, until, gradually, intellectual man began to expand. With this expansion came thought of comfort and desires for better things; in short, ambition. Now the family began to be a gathering separate from the herd, the migratory habit ceased, places of settlement became permanent, and the former herds populated towns or became fixed settled bodies of individuals, divided into families of father, mother, and children.

Each family became a separate and distinct body, but

bound together for mutual protection. The strong still endeavored to triumph over the weak, and the weak were probably more numerous than before; the new feeling which had grown up—family love—made the stronger members protect the weaker. But outside the family were others who would still look with longing eyes, and covet a neighbor's wife or servant, house or land—feelings and longings which, in their former condition, were gratified by simply taking possession.

Now the integrity of the whole demanded that the weak should be protected, and laws were made sufficient unto the time. Here, then, was the first step in the establishment of crime. Acts which now come under the bane of society, were looked on with indifference by the herd; but the enactment of law made these acts crimes. Increasing intellectual development has, of course, added to the laws, and with the increase of laws so has crime kept pace. Not, however, that there are any more acts committed against society, but the moral tone of society is so far elevated, one generation above the other, that acts condoned in the days of the father become crimes during the generation of the son. As education and enlightenment walk hand in hand, it is not to be wondered at that crime should increase. Neither is the increase a matter for regret; but rather a healthy sign of the times.

But, as in ages when there were no laws, and down through the centuries that intervene until our time, there have always been people who would resort to measures not to be commended, to obtain the means of existence, or to gratify desires. On this point much has been said. Reasons have been given, diverse in many particulars, why we should have criminal classes.

The physical reasons for crime can generally, however, be brought into a small group, and considered under two heads, viz.: heredity and environment. The moral side, as I have already said, is of great importance, and admits

of much argument. While we will not enter into it, we may say that the strong point of most writers on the social question is the statement that the misery and distress found in this world are due to the ignorance, neglect, or contempt of the natural rights of man.

The influence of heredity in the production of crime is more than doubtful. So great is it considered by some, however, that the enactment of laws to prevent the propagation of children by criminals has been advocated. It would be very difficult to prove that man bequeathed to his offspring tendencies, either criminal or otherwise, as a natural heritage. The weight of evidence is, to my mind, against the truth of such a claim. We have no less an authority than Superintendent Byrnes, of the New York Police Department, as published in the *North American Review* for July, 1894, as follows: "A good deal is said nowadays about the causes of crime, and crime as a hereditary disease. I have observed that most of the crime committed in New York is due chiefly to two causes—drink and environment." . . . "I have seen men so corrupt that it seemed to me as if a tendency to commit crime must be in their blood; but, on the whole, I do not put much faith in the theory that criminals are born with an irresistible tendency to evil-doing. I know that children of criminals are very apt to become criminals themselves," and he then goes on to show that, as their homes are the resorts of criminals, the children very naturally fall into vicious ways.

It is supposed that certain physical attributes are handed down from one generation to another. Features of the father are reproduced in the son. Physical characteristics are said to distinguish races. The ethnologist claims to be able to tell by the shape of a skull or a jaw-bone the race to which its owner belonged. In families not only shapes of heads and features, but color of hair, eyes, and general build, are said to distinguish one family from

another. If we believe in the theory of the separate origin of races, it is easily understood why differences should exist in their anatomical peculiarities, but even here we find diversity of opinions. As Prof. M. J. Weber said as long ago as 1830, "There is no proper mark of a definite race-form of the cranium so firmly attached that it may not be found in some other race." (See "Crania Brittanica.")

The resemblance between members of the same family will depend on two things, namely—First, the imagination of the observer; and, second, on the fact. Great differences of opinion will exist with different observers. One will declare two brothers look alike, notwithstanding one is short and the other tall; one is dark and the other light; one stout and the other slim; and, in fact, their appearances may be entirely opposite. On the other hand, persons between whom there exists no relationship whatsoever, except that they belong to the same race, frequently bear to each other the most striking resemblance. This resemblance, which is often so marked as to make it difficult to distinguish one from the other, may be due to the effect of mental work on the facial expression. As certain thoughts will be expressed by certain movements of the face, as grief, joy, recognition, etc., which movements are due to involuntary or reflex muscular actions, so people who are accustomed to think in certain lines, after a while become to look alike. Physicians resemble each other; lawyers, cobblers, tailors, and, in fact, nearly every class of professional men and artisans, have certain actions and expressions, due to their habits and ways of thinking, that distinguish them one from the other.

What is more likely than that this resemblance should be greater in families, between brothers and sisters, than among strangers? What could be more natural than that a son should resemble his father in feature, gesture, and general action, for this reason alone? A very excellent paper on this subject, well worth reading, will be found in

the "Popular Science Monthly," Vol. XLV, No. 3, the title of which is "On Acquired Facial Expression," by Louis Robinson, M. D.

So it may be said that physical resemblance between members of the same family is largely imaginary; and where it does really exist, it is due to training and not to heredity.

We must, of course, admit that heredity does play an important part in the development of certain of the lower animals. But it does not seem to me that there is any analogy between them and man in this particular. Certain physical characteristics are made to appear in certain breeds of animals, generation after generation; but it is only with the greatest difficulty that it is kept up, necessitating the selection of animals for propagating, and even with the greatest care, the breeds tend to return to their original conditions. It may be that, by proper selection, we might be able to apply these same rules to man, but I do not think it at all probable that such a procedure would be attended with any degree of success. Certain it is, that where certain customs have been followed, both as religious rites and otherwise, as, for example, among the Arabian, Jewish, and other races, where circumcision has been practised since time out of memory, the effect has not been different on the offspring than in races where it has not been done. The Jewish race, especially, where circumcision is practised as a religious rite in the early days of infancy, and where selection is made in mating, at least to a certain extent, would seem to offer the conditions necessary, yet we find these children not different at birth from other children.

If this is true of physical attributes, how much more unlikely that mental attributes should be inherited!

Physical characteristics exist at birth; but the mind is entirely the result of development, and it develops absolutely as it is trained. If we expect criminals to beget

criminals, so, on the other hand, we should expect highly-educated and intellectual people to transmit to their offspring greater intellectual qualities. We know that this, as a rule, is not the case. Nobility of character and intellectual qualities do not, necessarily, descend from father to son as a natural heritage. The only influence such a parentage can have is the advantage of training and development, and the force of example during the developing period of the child's life. The same may be said of the criminal. The only direct result of criminal parentage is the criminal training during childhood. The individual who neglects the moral training of his child, expecting of his offspring great results because of his own greatness and goodness, will find, often too late, his error. Proverbs xxii, 6, says: "Train up a child in the way he should go, and when he is old he will not depart from it."

"Heredity" in this connection is, therefore, a misnomer. In its place should be put the word "environment," which includes everything else. Impressions made in childhood by force of example, teaching, and general surroundings, are the factors which determine, as a rule, the course of the adult. In the diary of a friend, lately deceased, was found this entry: "Every word spoken, and every gesture made in the presence of a child, has its influence in the formation of that child's life." Gay has said: "The hand that rocks the cradle rules the world." The worship of wealth and position, the teaching that might makes right, —all these become to the growing mind influences which determine its future course.

But environment, beside embracing parentage, training, and all external surroundings, includes also factors within the individual himself which affect the teachings, whether they be for good or evil.

The blind child, for example, would be entirely unaffected by its surroundings if it had no other means of communication than vision. Environment, therefore, must

include the avenues through which education is acquired. What are these avenues?

At birth the human being is simply a creature of physical development; intellectual development comes later. The child of the most refined parents can be made a criminal, and so the child of the most depraved parents, if taken immediately at its birth, can be developed into the highest type of human being. I have no doubt, that if a child were allowed to grow to manhood without mental training of any kind, he would develop into just such an animal as our earliest ancestors are said to have been.

Education begins in infancy, through our mental appreciation of objects around us, the existence of which is demonstrated to us through the medium of the eye. Other avenues contribute to the result, as the other so-called special senses. In the absence of vision, as in children born blind, or who lose their sight through accident or disease, these avenues can be made to fulfil the natural function of the eye. Instances are not wanting where people who were blind have become highly educated through the sense of touch. Here one special sense, in the absence of another, is made abnormally sensitive. This, however, is unusual.

Education in the earliest days of life is purely imitative. We speak the language of our parents because that is the one we most often hear. A child, born of English parents, transferred at an early period of life to German surroundings, would become, in speech, action, and thought, German. The characteristics of his forefathers would be entirely foreign to him. Not only this, but his physical peculiarities, if nations can be said to have any physical peculiarities, would be German. So a child born of refined parents, put under the influence of less refined people, would develop on a par with his surroundings. If, perchance, in his manhood, he should rise above their level, it would be purely accidental.

One takes pride in the traditions of his country simply because these traditions have been dinned into his ears since his earliest recollection. The native-born American, educated from infancy on foreign soil, would fail to be fired with enthusiasm at hearing for the first time the martial airs of his native land. The love of country, like the love of home, is purely the result of education. G. H. Lewes, in his life of Goethe, says: "It would be nearer the mark to say that man is the architect of circumstances." But this can be only partly true, and a very small part at that. The circumstances over which man has control are very few, while the forces over which he has no control whatsoever, encompass him on every side. Verily, man is the creature of circumstances. Surrounded only by moral teachings, seeing only beautiful objects, subject to nothing which can mar the harmony and peace of life, it is impossible that moral crimes should exist.

Unfortunately, we are so situated that this ideal life cannot be led. The child of the most refined parents has presented to him objects of the grossest immorality. He is forced to compete with vice in every form, so that the influence of good teaching is counteracted by the influence of bad. These influences which are bad may come from external forces, or from defects which are in himself. I shall not consider here the influences which may come from physical defects generally, but will confine myself to influences which come from defective vision.

The young infant takes very little note of retinal influences. Soon, however, the child begins to notice objects around him, and, depending on the perfection with which these objects are portrayed on his retina, does he sluggishly or rapidly appreciate what he sees. The eye is simply a camera which pictures surroundings perfectly or imperfectly in accordance with the perfect or imperfect condition of the organ itself. Astigmation, for example, when it exists, is supposed to be congenital. The axis may be

any direction from vertical to horizontal. When it is oblique, and the axes of the two eyes parallel, objects viewed must be tilted. That such a condition occurs is shown by the fact that, when in after years the refraction is corrected, the correcting glass will cause objects to tilt in the opposite direction, for the time being. Here, then, on the threshold of life, occurs an element of danger. Some mental effort, involuntary no doubt, is necessary to enable the possessor of such a condition to see the world as others see it.

Excessive amounts of hypermetropia call for increased muscular effort beyond the normal, to accurately portray on the fundus of the eye objects viewed. Muscular effort, accommodation as it is called, is necessary for any one to see clearly and distinctly. Vision is never in any sense passive. All of our special senses, when employed to their fullest extent, require concentration of force. We hear sounds vaguely; suddenly a familiar tone strikes our ear; we stop. We concentrate all our energies in the act of listening. The same with smell and touch. We feel in our hands a substance,—a fabric, perhaps. We feel it carefully, shutting out everything else; we concentrate all our energies on the sense of touch. So with vision. Our eyes take in many things as we aimlessly gaze about us. But, when we wish to see distinctly, we ignore everything else for the object looked at. This may be called that indefinable force, mental concentration, but it implies involuntary muscular action as well. The individual with imperfect organs of vision, labors, therefore under a disadvantage, and if he sees as well as others it must be with greater muscular effort. He uses up force in trying to get perfect images that the person with normal eyes has left for coming to logical conclusions.

Knowledge is acquired through impressions made on the organs of special sense. It is through these channels that we obtain absolute knowledge. Inferences can never

be designated as facts. Facts are what we see and hear. The ordinary witness in a court of law, except it be in expert testimony, would never be allowed to give opinions; only what he absolutely knows, from what he actually sees and hears, is admitted in evidence. How essential it is, therefore, that he should have these organs in a condition capable of seeing and hearing correctly. Great interests, —a life, perhaps,—may depend on their perfection. Even judges and experts will vary in their thoughts and opinions, depending on the precision with which they grasp facts as they are seen or heard. Opinion is merely the expression of thought, and thought the expression of sight and hearing. There are very few people in this world who will doubt what they actually see; hence the saying, "To see is to believe."

The mechanism of vision is simple in so far as the eye itself is concerned. Rays of light emanating from luminous bodies pass through the air, penetrate the eye, and on the retina is depicted an inverted image of the object viewed. If it were here appreciated, the world and all thereon would seem inverted; but by some means, to physiologists unknown, this picture is taken up by the optic nerve, carried to the brain, and there seen in its upright position. If the eye is imperfect, the image must be depicted on the retina in a distorted condition. If there existed in the eye itself no compensating power, this distorted impression would be carried to the brain in a condition unlike the original. It is evident that the possessor of such an eye would have, at best, a very imperfect idea of things as they actually exist.

Fortunately for mankind, the eye is provided with a compensating apparatus in the muscle of accommodation. A common example is this: A person enters a room; on a table is an object which to the observer seems an image of some horrible thing. So hideous is it that it inspires a feeling of fright. He fastens his eyes on it, and gradu-

ally its irregular outlines straighten, and the fear-inspiring object becomes a masterpiece of most beautiful and exquisite statuary. The feeling of fear becomes one of admiration. But the real appreciation in this individual requires time. If a series of objects pass rapidly before his eyes, it is evident that, in the time required to see all distinctly, much must be lost. If it should so happen that the error in such eyes as these is greater than the power of compensation, a correct appreciation of objects viewed would be impossible. It is difficult for one not so situated to fully appreciate the condition of mind engendered by such a defect. Yet as much is expected of this being as of his more fortunate fellow.

The result of such a condition may be varied. Some may be content to accept the fact as it is. They would be designated as stupid. Some, by persistent exercise of will, may develop their accommodative powers sufficiently to overcome the trouble, but it will be at the expense of some other portion of the human machine. Some, battling against these odds, may not be so happily constituted as to be able to exert the required force, and fail.

The first accepts defeat and says no more about it. The second achieves success at the expense of general health. He is irritable, dyspeptic; he is said to have an excess of bile, and so on. The third, which will constitute a large proportion of people so affected, will writhe under defeat. They will be the disappointed people. Unwilling to accept places in the lower walks of life, they try to compete with their more favored brothers. But they are handicapped, and failing in the race because of the unequal struggle, they seek to gain by foul means what they cannot get by fair. And so, from childhood up, the world to them is never quite right. They are taught to think white when they see black. What they are told is straight, to them seems crooked. How can

an individual so weighted be expected to be like other men? To him things are never as they seem. He thinks the whole world is wrong. He becomes a criminal from necessity.

Here, then, it seems to me, is the explanation of the statement that education increases crime. Education gives to its possessor an advantage over his less favored brother. It gives to a community a refinement which causes it to include in the category of crime, actions which would be looked on less harshly were the standard lower. It makes also the competition greater, and, as the race is always to the swift, the man who is weighted with physical disabilities, must, unless he is capable of unusual effort, bring up in the rear. Nothing is so disheartening as continued failure and defeat. To borrow an expression from one of our evening papers, "If nothing succeeds like success, nothing fails like failure."

Through the kindness of my friend, Dr. Van Santvoord, and the Board of Directors of the Society for the Reformation of Juvenile Delinquents, I have been enabled to make some observations in the institution they so ably manage. I desire to express to them my thanks. The reformatory is situated on Randall's Island, and receives children from certain parts of the state, who are committed by magistrates for offenses, criminal and otherwise. I expected, of course, to find many cases of defective vision among so many children, taken, as they are, from the lower classes. The ages range from eight to nineteen years, the average of those examined being about fourteen years. The authorities were very anxious to have the matter inquired into, and sent me first those who complained of their eyes, or who were thought by the teachers to have defective vision. Any one who has had dealings with children in institutions will know that, to ask the children themselves who among them had trouble of this nature, would be to elicit an affirmative

answer from nearly every one of them. But a careful canvass of the school by Mr. C. W. Manchester, the principal, brought forth seventy-five children who either could not see well, or else complained of their eyes paining them. Of this number, 51, or 68 per cent., were found to have defective vision.

The test of vision was the ability to read the line on the test-card which, at a distance of twenty feet, forms with the eye an angle of 5′. I think that this test is low; for, in an experience extending over a period of thirteen years, in the New York Eye and Ear Infirmary and the Manhattan Eye and Ear Hospital, where together, perhaps, thirty thousand or more patients are treated yearly, I have been forced to the conclusion that normal acuity of vision is greater than 20–20ths, is not less, in fact, than 20–15ths, and it is not unusual to have nearly, or quite, 20–10ths. I think, therefore, that, while normal vision may range between 20–20ths and 20–10ths, a vision of 20-30ths may safely be said to be defective. My work was interrupted because of my own lack of time, but I was enabled to make an examination of some twenty-six more children who had never complained in any way of their eyes, and who were not supposed to have any trouble of this nature.

The result was certainly surprising, for of these twenty-six, fifteen, or 57.5 per cent., were found to have defective vision. I see no reason to doubt that this would have been the result if the entire institution were gone over carefully, and every child examined. There was no discrimination in the selection of these twenty-six children; they were taken one after another as they happened to come. Of the total number—101—examined, 65, or 64 per cent., had defective vision. The small number examined is, of course, not sufficient to draw conclusions from, but it is, at least, suggestive. I do not know how this would correspond with the conditions

existing generally, in public schools, and institutions not reformatory, but I do know that many of the defects existing could be easily remedied if oculists were attached to all institutions where children are educated. The board of directors of the House of Refuge have, I am glad to say, since appointed a capable oculist, and I am sure good results will come from it.

We have now considered the origin of crime in the beginning of history, and the causes that perpetuate its existence and govern its increase. We have endeavored to confine ourself, as nearly as possible, to the physical side. The moral side is not less important. "Man's inhumanity to man makes countless thousands mourn;" but if we took up the subject in full, going into details on every side, the discussion would be never-ending. We have considered that heredity *per se* plays an insignificant part in the production of crime; that environment predisposes man to good or evil, but that the all-important element is education, and that education may be acquired with ease or difficulty, according as the avenues through which it is acquired are perfect or imperfect. We must, therefore, consider finally how the disastrous effects of imperfect vision on man's intelligence can be combated.

Optical defects may be congenital or acquired, permanent or temporary. Many of the permanent defects are congenital, but some are acquired in the early days of life, and are avoidable. We find in Noyes, on "Diseases of the Eye," a statement of over 3,000 cases of blindness in children, copied from observations made by Magnus, in European institutions, 23 per cent. of whom were incurably blind from ophthalmia neonatorum. If the number were known, of children with imperfect vision as the result of sore eyes in infancy, the figures would be astonishing.

Sore eyes in infancy, ophthalmia neonatorum, is, I think, absolutely preventable. It is quite the rule to find

more or less conjunctivitis in the newly-born infant among the poorer class of people, in the tenement-houses. Recognition has been taken lately, in requiring midwives to call in physicians when the baby's eyes are sore. Within the past year, two midwives at least have been given cause to remember that such a law exists. Not only midwives, but physicians do not give to this matter the attention that its importance demands. It should be the care of the medical attendant at the birth of every child to personally direct the washing of its eyes. Absolute cleanliness here prevents many disastrous results. Or if, despite every precaution, severe inflammatory conditions should prevail, it is much better to consult an oculist in the early stages, before irreparable damage is done, than to wait until the cornea has perforated, the iris prolapsed, and the eye perhaps been lost.

With congenital defects early attention will often start the child on the right path; while delay may interfere with the chances of success later on. For example: Congenital cataract, if operated on early, exposes the retina, and enables it to perform its function as nature intended it should. The question whether amblyopia results from disuse, is one of many sides, but that defective eyes gradually improve when the defects are corrected, is too common an occurrence to admit of dispute.

The advice of family physicians will often determine the fate of these little patients, and it is therefore well that they should especially interest themselves, and study the possibilities in each case. I call to my mind now a child with congenital cataract in each eye, whose only vision was perception of light. That is, the retina was sensitive, so that the distinction between light and dark was perfect. The child was otherwise healthy, and the chances of success from operative procedure were very good. But physicians are not prophets, and because of our inability to give a positive assurance of perfect

success, an operation was refused by the parent. An operation could not have made the condition worse, as perception of light was all that existed, which for any practical purpose was useless, while it offered every chance of success. As the result, this child was condemned to years of blindness. Our civilization has not advanced far enough to deny the parent the right to decide its offspring's fate in such a case, but, better would it have been for both mother and child, if some less interested person, as the family physician, could have decided the question.

Preventable defects should be prevented, and congenital defects remedied, as far as possible, in the early days or months of infancy. In infancy it is not possible always to detect the defects known as refractive errors, which are by far the most numerous and important; nor, were detection possible, would the treatment be practicable. But in childhood, that is, after two years of age, not only is detection possible, but easily accomplished, and correction, while sometimes difficult in early childhood, can be, and is, successfully carried out. It is better to correct glaring defects and deformities at the earliest possible age. It is at this time that children first begin to squint. Simple correction with glasses now may not only avoid the necessity of an operation later on, but may preserve useful vision in an eye which would, perhaps, become amblyopic. Young children who blink the eyelids, those whose eyes are over-sensitive to the light, those who constantly hold objects too near in order to see them clearly, or who show evidence of any condition which is sufficiently noticeable to attract attention, should receive immediate treatment. The fallacy that children outgrow certain conditions is not confined to the laity. But the outgrowing of myopia, or of periodic squint, means, in the first instance, the passage from benign to malignant; and in the second, from occasional to constant.

The place, par excellence, where the most harm is done, is undoubtedly in the school, and the public school especially, for here is where most of our children, at least in this city, receive their education. I feel that too much cannot be said on this point. The massing of children together, under one system of teaching, without first paying attention to their physical abilities or disabilities, is wrong. We have done much in this city in the improvement of our school-houses; they are better lighted, better ventilated, and in better hygienic condition generally, than they were. It is safe to say that they will continue to improve, for the people generally recognize the necessity of education. But, notwithstanding the improvements in hygiene, in books, in methods of teaching, and all, absolutely nothing is done to see that the avenue through which the knowledge is acquired is normal. I am informed by teachers in schools that it is very common to have children who are unable to see the blackboard, even from the front row of seats. Some children lose a large part of their time from headaches due to defective vision, or from perfect vision maintained under difficulties. Not only this, but the nervous, irritable condition of many school children, and of teachers, too, for that matter, can be directly traced to defective eyes. The number of children affected in this way, I have no means of knowing, but I think an examination of the eyes of public school children would show most surprising results. How can this evil be overcome? Nothing is simpler. I advocate the examining of the eyes of all children upon entering the schools. We demand that every child shall show evidence of vaccination. A certificate of vaccination is required before the application is even considered. It is equally, and perhaps I might say, even more, important, that a certificate of ocular perfection should be required. We cannot cure ocular defects, always; we cannot make a myopic eye shorter, nor can we change the shape of the cornea; but we can correct

refractive errors with glasses, and put the possessor of eyes optically imperfect, on an equal footing with his more fortunate fellow. We are met here immediately with an objection. Parents do not want spectacled children, and their objection is reinforced by physicians who, certainly for no other reason than lack of knowledge, do not believe in children wearing glasses.

Not long ago, one of the leading medical journals of this country noticed with regret the growing tendency to put spectacles on children. It is not a thing to be regretted; it is rather a healthy sign of enlightenment and advancement. The growing demands of civilization necessitate one of two things; either the possessor of defective eyes must put on glasses, to make him the equal of his fellow-man, or he must take a back seat. The objections of parents when forced to consider these two alternatives, will speedily vanish. Of physicians, we can only ask that they give the subject careful consideration. Boards of education should have attached to them oculists whose duty it should be to examine at regular intervals the eyes of all children in schools. They should report to their superiors and suggest remedies. The question of expense to the community should be of no consideration whatever. The matter should be out of the hands of politicians entirely. The comptroller of the city of New York may object to appropriations for public school purposes, on the ground of economy, but the question of economy is not considered when he subjects the city to unnecessary expense to gratify his ambition and increase his salary. There is no economy in stinting the board of education. It is absolutely certain that the more we spend on the public schools, the less we shall be obliged to spend on our prisons and almshouses.

Not only public schools, reformatories, and other public institutions where children are taught, should have the eyes of inmates examined regularly, but private institutions as

well. Let parents fully realize, what they already know in a general way, that perfect eyes are absolutely necessary to success in the competition of this life, and the objection to glasses will, as I said before, soon vanish. After school life the condition of the eyes should determine the choice of occupation. Imagine a boy of sixteen years with a myopia of twenty dioptres, addressing letters from morning till night. I have just such a case in view. The father of this subject is a bartender and has perfect eyes. The son is a clerk and has very imperfect eyes. This is one of the inconsistencies of life easily avoided. Better for this lad had he been a laborer or farmer, or at least in some position requiring less use of visual organs. I consider ultimate blindness almost inevitable in his case.

The law, happily, has interfered in some cases of defective perception, for the public good. The examination for color-blindness has deposed many men from positions requiring the use of normal eyes, and has compelled them to seek other means of earning a living. It is not too much to ask the people to enact laws for their still further protection, by enforcing ocular perfection. The existence of myopia should always, I think, determine the occupation, especially where the amount is great and the tendency is toward progressive myopia. In low degrees properly fitting glasses may be all that is necessary. They should be insisted on, however, by medical men and employers. Other conditions, as excessive amounts of hypermetropia and astigmatism, should not debar one from choosing almost any occupation, provided the eye is made normal with glasses. In astigmatism, even the smallest error should be corrected. Excessive astigmatism, uncorrected, necessitates poor vision. The result of poor vision is that one sees slower than other people. With small errors, however, perfect vision is not only possible but generally attained. The person whose perfect vision is maintained under great strain is apt to be abnormally sen-

sitive. The constant sense of alertness necessary to keep track of everything, a condition which is not necessary to the one without ocular defects,—and so impossible of attainment that it is not attempted in the possessor of marked errors,—begets such a condition of general hyperæsthesia that is often never absent, as to render the person so afflicted, irritable, excitable, and of uncertain action generally. Who can wonder that this class of people commit all sorts of acts for which they are not, morally, responsible?

And now, in conclusion, let me sum up in a few words the ideas I have endeavored to convey. It is very probable that the human race has altered very little, if at all, since the beginning. There is nothing but theory, and that very far-fetched, to prove the opposite. At any rate, there has been no change since the earliest time known to history. The surroundings, however, are very different. From being creatures of purely physical development we have become creatures in whom intellect predominates. Our anatomy has not kept pace with the physiological requirements. Spectacles are not evidence of growing physical weakness, but rather of increased intellectual growth. Our eyes are merely the principal avenues through which intellectual food is acquired. If all individuals were alike, and all eyes imperfect, the conditions would be more nearly equal. It happens that very many eyes are as perfect as it is possible for eyes to be, and they confer on their possessors an immense advantage. Man always takes advantage of his weaker brother, who is obliged to resort to other measures in self-defense; if the measures he employs are legal, well and good; if they are illegal, he becomes a criminal.

Do not misunderstand me. Crime has many causes, many features. Defective vision may not be in itself alone sufficient to make a man a criminal, but its influence is great and deserves careful consideration.

DISCUSSION.

DR. H. D. CHAPIN.—I have listened with great pleasure to this paper. It has afforded me much food for thought, and I may say it has stimulated me so in certain lines that I should prefer to think about it a little before I am prepared to speak at length. The subject of the increase of crime is one of the greatest interest. Some years ago, I looked up the statistics, not only regarding the increase of crime, but also of insanity and pauperism—the defective and delinquent classes generally—and gave the results in an article entitled "The Survival of the Unfit," which showed very conclusively that these classes of society are increasing more rapidly than the general population. That fact alone is enough to render a paper of this kind of the greatest interest.

The relative importance of heredity and environment in the production of crime is one of great importance, and I must say that the paper this evening has placed the relationship of these two factors in a new light to me. I am glad to hear Dr. Van Fleet express the opinion that environment is so important, indeed, much more so than heredity. I like to think this is so. Heredity is certainly beyond our reach, while environment we can modify to a certain extent. I believe, from the physical aspect of the subject, that there is some truth in what the doctor says. I have no doubt that the influence of heredity has sometimes been overestimated, and that certain facts that have been attributed to it can more truthfully be credited to environment. I was first led to this view some twelve years ago, when I was placed in charge of St. Christopher's Home, then in this city. Its inmates were twenty-five little girls, ranging in age from two to three years up to ten. They were taken from the worst tenement districts in the city; their parents were either dead, or drunkards, or ne'er-do-wells. I spent a good deal of time in studying these children; I regulated their diet, their studies, their sleeping hours, etc. For the first few years, there was considerable skin disease among them, eczema, etc., and all forms of catarrhal trouble and scrofulous conditions, but as time went on, their physical condition improved. In a word, their physical defects became eradi-

cated by their environment. I have observed this same thing in certain families. I think, physically, we can do almost anything with children by means of favorable environment. I am not so sure that we can do so much morally. I remember reading a little volume, which probably many of you have seen, which gives the history of a certain family for four or five generations, and almost every member of that family turned out to be a prostitute, an inebriate, or a criminal. Of course, such cases may be due to training and surroundings quite as much as to heredity. While I cannot go as far as Dr. Van Fleet has in his paper, yet I welcome his views as timely, and I hope they may be verified. So far as children are concerned, I regard environment as a factor which will prove most powerful for good in our hands.

When we come to lesions of the eye in children, there is only one point to which I desire to refer, and that is, the occurrence of ophthalmia neonatorum. I, as well as many others, constantly have children brought to the dispensary who are suffering from this affection; the eye is filled with pus, the lids swollen, so that it is impossible for me to get a view of the cornea. What is to be done? We all know that unless that child receives absolutely the best of care, blindness will be sure to ensue. So far as I know, there is nothing to be done for that child but to let it become blind. We have eye institutions in this city, but they refuse to take such cases. It will do no good to tell the child's mother, who is usually a poor, ignorant woman, to draw the lids back every fifteen minutes, and wash out the eye. She perhaps has half a dozen more children to look after, as well as the housework, and she simply cannot carry out the treatment. What we urgently require in this city is an institution where these patients will be admitted and cared for.

I quite agree with the statements made by the reader of the paper regarding the condition of most of our schools. Some years ago, I made an inspection of about twenty of the public schools, most of them on the lower east side of the city. I found that the primary school children were invariably given the poorest accommodations. They were kept down on the ground floor, and the seats were so arranged that the light from the windows was directly in

the children's eyes. Many of the rooms were lighted with artificial light.

DR. J. COLLINS.—I am sure the reader of the paper may be congratulated on the felicitous manner in which he has presented this subject. It is an enormously large one, and difficult to discuss. In the beginning, I may say that I differ with him in almost every point of contention he has brought up, with perhaps one or two exceptions. First of all, in considering the causes of crime, we cannot afford to leave out the cosmic element nor the biological element. If we do not take into consideration the first of these, we are reckoning without our host. It has been proven by criminologists, and the fact was brought out at the last meeting of the Anthropological society in Paris, that the cosmic influence in crime is not to be sneered at. We see this in the newspaper accounts of crime in our own city. During the winter, crimes against property are common; while in the summer, crimes against persons—murder, suicide, rape—are more frequently committed.

When we come to education as a factor in crime, the reader states that it has been proven, or at least asserted, that education tends to increase crime, and so it has; but he failed to designate what sort of education. Intellectual education tends to increase crime, while manual education will diminish it. This is not my *ipse dixit*, but the consensus of opinion of all who are interested in criminology. If we take children with criminal antecedents, and teach them to use their hands, teach them handicraft, a trade, and then send them out to contend against their fellows, we are very likely to decrease crime. If, on the other hand, we pay a similar amount of attention to their intellect, which nature has already warped, we may give them just enough perspicuousness to have the acumen to commit crimes.

It surely is a novel thing to listen to a paper on crime, in which heredity is considered, where no mention is made of the works of Weissmann and others, who have long and fruitfully labored in this field.

The fact that circumcision, which has for centuries been practised by the sons of Abraham, does not result in the birth of children without foreskins, or that cutting off lambs'

tails is not followed by the birth of lambs without tails, should not be confounded with heredity, which is something much more obscure than the transmission of acquired characteristics, and whereas the reader of the paper has scarcely touched upon it, it is scarcely worth while entering upon a discussion of heredity, from the biological standpoint, unless we can go into it very fully. If, in the consideration of crime, we do not regard its influence on the germ plasm, then we do not analyze the one important factor of heredity. If environment is a more potent factor in the production of crime than heredity, and we can combat the effects of the former, why, I ask, is it that crime does not decrease? Statistics show that it is on the increase. Contrary to the view expressed by the author of the paper, I think it has been shown very clearly by accurate experiments and observation that members of families *do* resemble one another. Twins resemble one another, and there are very good reasons for this. The peculiar shape of the nose, in the Hebrew race, is as distinctive to-day as it was ten centuries ago.

As regards criminals begetting criminals, this is strikingly illustrated in the annals of the Gude family. Inspector Byrnes, in his book, cites numerous examples where criminals have sent their children into convents, and yet the sins of their fathers fastened upon them, and manifested themselves later in life. I have in mind one case, the keeper of a brothel in this city, who sent her daughter, at a tender age, to a Catholic institute. She came out when she was twenty-one years of age, and the first thing she did was to marry a book-maker, and now she has taken her mother's place. However, to cite individual cases is of very little use, but I might speak of one as regards environment: the bastard son of the first Napoleon was brought up under excellent surroundings; his environment was of the best; still, he became notorious, and was constantly a fugitive from justice. I do not agree with the statement, made by the reader of the paper, that if an American or English child is brought up in Germany, or *vice versa*, he would possess, when a man, the characteristics of that nationality with which he had been reared. I believe that a German boy who is raised in this country is still imbued with that spirit of intense persist-

ency and patient application which is a characteristic of his race, and not of ours.

I should like to inquire of Dr. Van Fleet whether criminal statistics show that the large proportion of that class have bad eyes. A criminal may have defective eyes, but he is also defective otherwise; he has defective smell, defective taste, defective sensory conduction, he is unable to feel the pangs of sorrow or of repentance. In the statistics given by the reader, he tells us how many children he examined, and in how many cases he found the eyes defective, but he does not say how many children were in the institution; so that we can form no idea of how large a percentage had defective eyes. He claims that the more frequent prescribing of glasses for children is a mark of intellectual advancement; on the contrary, I hold that there is too much indiscriminate prescribing of glasses, but not by physicians; it is done by the "professors" in Grand street, the Bowery, etc.

In conclusion, Mr. President, I do not consider that the view which relegates heredity, and attributes to environment the all-important factor in the causation of crime, should receive, even apparently, the approbation of this deliberate body.

DR. F. PETERSON.—When the author of the paper asked me to say something on this subject, I told him that I should be glad to hear the paper, but that I did not know anything about it. I was rather surprised to find that so much stress was laid upon defective vision as a cause of crime. I have been under the impression that most people had defective vision—certainly in as high a per cent. as the author found in the children he examined. Among the Arabians and Egyptians, who almost without exception have some eye trouble, we find that crime is extremely rare. Dr. Van Fleet's ideas regarding the importance of environment in the production of crime, are much more radical than my own. He seems to deny the potency of heredity entirely. If that is so, then Weissmann, Galton, Lombroso, and their disciples, and others, may as well give up their studies in this field. Personally, I think there is no doubt that heredity is infinitely more potent than environment in the causation of crime.

DR. J. E. WEEKS.—I have listened with much interest

to Dr. Van Fleet's paper, and have failed to find in it any allusions to the title that are at all commensurate with the length of the paper. The history of the human race and of the criminal classes is interesting, as are also his strictures of officials in regard to the poor lighting facilities in our public schools, but when the title of a paper is announced, the listener expects to hear something on that particular subject.

So far as defective vision is concerned, competent authorities state that it exists in at least two thirds of the human race. Astigmatism is probably present in that proportion of human beings. The other day I examined the eyes of a young woman and found them perfect so far as optic principles are concerned, and it is probably the only case of its kind I have seen in my office for a number of years. It does not surprise me at all to learn the result of the doctor's examinations in the cases he refers to. It does surprise me that he did not make a more careful examination. Merely testing with a card the eyes of children, who are undoubtedly illiterate to a great degree, is a very superficial method of examination. Another thing that strikes me as peculiar is that if defective vision has much to do in the production of crime, in spite of our increasing knowledge in correcting defective vision, crime is on the increase. I believe that defective vision plays the same part in the production of crime that other defective senses do, and no more.

As regards the treatment of ophthalmia neonatorum, which was referred to by Dr. Chapin, I believe that such cases can be satisfactorily treated outside the hospital, if they are seen by the physician every day. Consequently, the necessity of having a special institution for such patients is not so pressing. At the New York Eye and Ear Infirmary the mothers of such patients are instructed to use certain precautions, and to return the patients for treatment each day, and under this plan in certainly not more than two or three per cent., and probably in not more than a fraction of one per cent., is there any loss or serious defect of vision.

THE PRESIDENT.—Although greatly interested in the subject which has been presented to us to-night, I will not give my own views regarding it, which in some respects

might differ from those of the reader of the paper. I simply wish to make this subject of contagious ophthalmia, and the admission of such cases into the hospitals, a little plainer, and to put the responsibility where it belongs, namely, not on the eye institutions, but on the people who control certain hospitals in this city, the commissioners of public charities and correction. The Manhattan Eye and Ear Hospital, and also the New York Eye and Ear Infirmary, treat all cases of ophthalmia neonatorum that come to them, and advise daily attendance; this, as Dr. Weeks has already stated, is generally sufficient if there is anybody at the patient's home who is at all competent to carry out the doctor's instructions with regard to cleansing the eyes. At the Manhattan Eye and Ear Hospital we devote one floor to cases of contagious ophthalmia. If we were to admit all that came to us, we should have to stop taking care of cataract cases, stop operating for strabismus and glaucoma —in fact, we could do nothing but treat ophthalmia neonatorum, and other contagious forms of eye disease. It is outrageous that in this great city there is no public hospital for the treatment of purulent ophthalmia, but this is not the fault of the profession. The subject has been presented to the highest authorities, but the suggestion was regarded as an impertinence, and resented with that high and mighty air which is peculiar to the politician. We should continue to agitate this subject in season and out of season, and constantly bring it to the attention of the authorities and men of means.

DR. F. VAN FLEET (closing the discussion).—It would be extremely difficult for me to answer all the remarks made by the gentlemen who have honored me by discussing my paper, and I can only repeat what I have said, namely, that in my opinion, heredity falls into almost utter insignificance when we fairly and squarely compare it with environment as a factor in the production of crime. Men like to fall back on heredity; it is an easy way to shift the responsibility for their wrong-doings. We have on our statute books a law which is commonly known as the "Baby Act," which is meant to protect minors, who, when they fall back on it, are said to "plead the Baby Act." So it is, I think, when a man falls back on heredity.

I believe that on a subject of this kind, the opinion of a

man like Superintendent Byrnes, who has had extended practical experience with the criminal classes, is far superior to that of a theorist. If we go way back to the ovum and the spermatozoa, we enter a region of theory from which we can draw any conclusions we please. I believe you can take any child that is well-balanced mentally and make of that child what you will. Even admitting the fact that the majority of ministers' sons do not come up to the standard of their fathers, that does not prove anything for heredity; it is rather an argument in favor of environment, because ministers are apt to devote so much time to training other people's children that they neglect their own.

Stated Meeting, November 1st, 1894.

THE PATHOLOGY AND TREATMENT OF INTRALOBULAR OCCLUSION JAUNDICE.

By WILLIAM HENRY PORTER, M. D.

This title, intralobular occlusion jaundice, is employed in contradistinction to the term interlobular jaundice, or that form in which the etiological factor is found to be a partial or complete occlusion of the extra or interlobular bile-ducts.

The pathology and therapeutics of this very common form of jaundice have never been elucidated completely, although Stadelman has described a somewhat similar condition.

Before the etiology and pathology of this particular form of jaundice can be discussed intelligently, a clear understanding must be had of the anatomical relation of the epithelial cells of the hepatic lobule to the bile capillaries and the capillary blood-vessels within the acini of the liver.

The chemistry and the physiology of the formation of the bile must also be accurately comprehended. It is not necessary, however, for a complete elucidation of this lesion, to consider in full detail all of the functions of the hepatic gland, but simply those phenomena which are concerned in the elaboration of the bile.

For many years it has been commonly taught by physiologists that the bile was formed in the liver out of the constituents contained in the blood and lymph as they flowed through the hepatic gland. What the exact modus operandi of formation is, has never been satis-

factorily explained, either in theory or by practical experimental demonstration.

A still older, and now almost obsolete, theory for the formation of the bile, was that the constituents of the secreted bile, after serving their purpose in the alimentary canal, were reabsorbed continually from the lumen of the alimentary tract into the capillary blood-vessels which enter into the intestinal portion of the entero-hepatic system of blood-vessels.

In passing from the intestinal canal to the blood stream, the bile salts and pigments lost their identity, because these substances cannot be recognized as such in the normal state of the system, when the blood in the entero-hepatic vessels is subjected to the tests by which the bile salts and pigments can be recognized. Nevertheless, it was claimed that the constituents of the bile were reabsorbed and passed in some unknown form from the intestine to the liver,—having reached the hepatic gland; the epithelial cells of the acini in the liver immediately commenced to pick up these unidentified substances and reproduced out of them the constituents which were to form again the biliary secretion.

That bile is made from the chemical constituents of the blood which are primarily proteid in composition, cannot be denied. But the reabsorption and mythical method of manufacture of the bile from unrecognizable compounds can hardly be considered as a scientific or satisfactory explanation. In fact, this old idea has been already pretty generally discarded by all the modern physiological chemists.

The experiments of Vaughan Harley, confirming as they do in a remarkable manner those previously made by von Fleischel, Kunkel, and Kufferath, all go to show that the constituents of the bile are not absorbed by the blood-vessels. Therefore, it can be positively stated that this theory of the formation of bile from reabsorbed bile is

erroneous beyond a question of doubt. And yet these old theories appear to be, in a large measure, the groundwork upon which much of the pathology of jaundice is constructed.

Therefore, for a more perfect explanation of this particular form of jaundice, a much more consistent theory of the elaboration of bile must be developed. The ana-

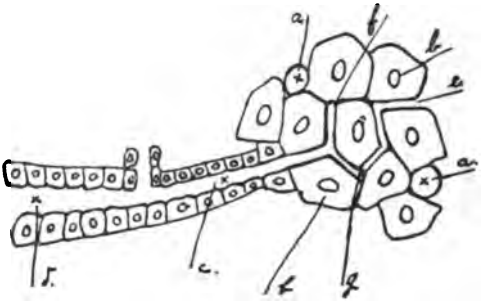

Fig. 1—Normal liver, showing relation of the hepatic cells, blood-vessels, lymphatics, and the biliary radicles within the acini (x 850 and reduced by ¼). a, intralobular blood-vessels; b, hepatic cells; c, neck of the biliary radicle within the acinus; d, bile-duct outside the lobule; e, lymphatic space; f, biliary radicle containing the hepatic cells, free and open into the neck; g, basement substance between bile radicle and lymphatic space.

tomical relations of the epithelial cells of the liver to the bile capillaries and the plexus of blood-vessels within the hepatic lobules, must be accurately understood to comprehend the formation of bile. Each acinus is composed of the glandular epithelium, blood-vessels, lymphatics, nerves, and a small amount of fibrillated connective tissue.

Each epithelial cell is composed of a mass of proteid protoplasm which may be considered as the epithelial lining of the blind and internal extremity of a bile capillary, as represented in Fig. 1. As a rule, there are two or more epithelial cells in each blind ending of the bile-ducts. This system of internal and blind extremities is the origin of the bile-ducts in the lobule, and which,

within the acini, constitute the minute capillary bile spaces.

These minute capillary bile spaces, or ducts, gradually and progressively converge, uniting into six or eight well-defined ducts which leave the periphery of the hepatic lobule at the same point that the interlobular veins enter the acinus. These converging capillary bile-ducts, located within the lobule of the liver, usually correspond in number and position with the blood-vessels as they penetrate the acinus.

Having reached the interlobular portion of the liver, these six or eight bile-ducts continue to converge, growing larger and larger, until they acquire microscopic dimensions, and, finally, they emerge from the transverse fissure of the liver as two common ducts, which ultimately join, and, with the cystic duct, form the common bile-duct.

As the blood-vessels of the portal system diverge and grow smaller and more numerous from the periphery of the liver to the center of each acinus, the bile capillaries in the opposite direction converge, growing larger and less numerous from the center of the lobule to the surface of the liver; that is, following the course of the flow in the two sets of tubes. The bile-ducts, therefore, may be said to run in the reverse order of the blood channels, and to lie in the sheath of the portal vessels or in the capsule of Glisson.

Aside from these large glandular epithelial cells which line the internal and blind extremities of the bile-ducts as they lie within the hepatic lobules, the smaller and the large-sized bile-ducts are lined throughout beyond the limits of the glandular epithelial cells, first, with a thin layer of somewhat flattened epithelium, the cells of which become more and more columnar in character as the tubes increase in size, until the largest are reached, where a very typical form of columnar epithelium is found.

The capillary or intralobular plexus of blood-vessels

ramifies all around in between these blind internal endings of the bile-ducts and their contained glandular epithelial cells. Consequently, these large masses of epithelial protoplasm which form all the lining there is to the biliary terminals, are also the true functionating or secreting portion of the liver. The only structure that separates these masses of protoplasm from the blood stream as it courses through the liver is the thin layer of basement substance upon which these epithelial cells rest, or what is the true basement wall of the biliary capillary, and the thin cells which constitute the walls of the capillary blood-vessels of the intralobular portion of the portal system.

The perivascular, or true lymphatic, spaces of the liver are apparently small, as compared with many other portions of the body, and are occupied largely by the expanded and blind external radicles of the bile-ducts.

Thus it becomes apparent that the blood, in its passage through the lobules of the hepatic gland, is brought into almost direct contact with these large masses of protoplasm that line the internal endings of the bile-ducts, and which constitute the secreting glandular epithelium of the liver.

By some observers it is claimed that these large epithelial cells in the blind extremities of the intralobular bile-ducts contain vacuoles. From these vacuoles they trace little tubular prolongations through the substance of the protoplasm to the surface of the cells. The external opening of this capillary tube in the protoplasm is supposed to come into direct contact with the thin membrane of tissue intervening between the lumen of the bile-duct and that of the capillary blood-vessel directly adjacent to the hepatic cell.

Through these capillary tubes in the protoplasm constituting the glandular epithelium, the chemical elements contained in the blood are supposed to be drawn into the substance of the secreting cells. Within the body of

these epithelial cells chemical mutations are wrought upon the proteid compounds by which the constituents of the bile are formed.

From the fact that the perivascular, or lymphatic, spaces of the liver are very largely filled by the blind internal radicles of the bile-ducts, the vascular capillaries are brought into almost direct relation to those constituting the bile-ducts and containing the epithelial cells of the liver.

From this anatomical arrangement, it becomes apparent, therefore, that with each afflux of blood to the liver, the hepatic gland must increase in size. At the same time, the pressure of blood against each individual mass of epithelial protoplasm is gradually augmented. This forces the epithelial cells against the outlet of the biliary radicle, and the protoplasmic mass, constituting the hepatic cells, imbibes the proteid and carbohydrate compounds of blood. This causes the epithelial cells to increase in size, and their protoplasm becomes more and more cloudy. This swelling continues until the biliary radicle is completely distended. While the size and pressure within the cells are continually increasing, the pressure of the blood within the intralobular capillaries against the cells is progressively decreasing.

When the resistance in the intralobular capillary blood-vessels is reduced to the minimum, the flow of blood away from the liver becomes more rapid, and the pressure against the hepatic cells is decreased. This enables the distended cells to draw back away from the opening of the ultimate bile-duct. It also enables them to easily discharge their contents into the bile-duct, accounting for the flow of bile from the epithelial cells into the primary ducts.

These physical changes in the blood-vessels and hepatic cells, however, do not explain the formation of the constituents of the bile, but simply illustrate its mechanical origin. For the chemico-physiological formation of the various constituents of the bile, the epithelial cells within the bile

radicles absorb oxygen from the blood at the same time that the proteids and carbo-hydrates are drawn into the same cells. This active oxygen, when once within the protoplasmic mass, comes in contact with the C H O or glucose compounds, attacks them in an active manner, and converts them directly into carbon dioxide and water, as

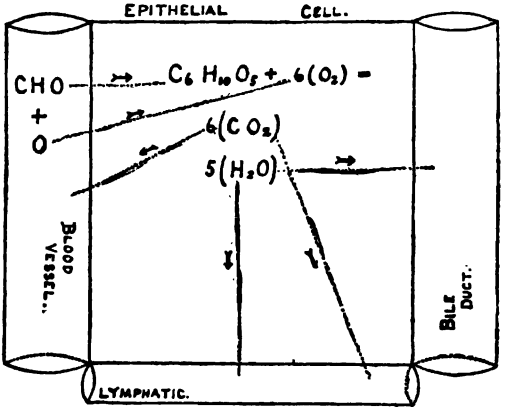

FIG. II—C H O Oxidation. Schematic drawing illustrating the oxidation process in the protoplasm of the epithelial cells.

illustrated by Fig. II. This oxidation of the glucose compounds generates a given amount of heat for each gramme transmuted into its final products—the water and carbon dioxide.

This constant production of heat in the liver is essential to maintain the peripheral nerve irritation in this organ which is absolutely necessary for a perfect working of the animal economy.

The water, which is separated by the oxidation of the $C_6H_{10}O_5$ or $C_6H_{12}O_6$ compounds in the liver, flows out of these hepatic cells into the bile-ducts and lymphatic spaces. The larger part passes out of the liver through the bile-ducts, supplying the water of the bile.

The carbon dioxide, separated at the same time, is

returned into the blood stream in the intralobular capillaries. A part remains in solution in this alkaline fluid, while the remaining portion is united with water and sodium carbonate to form sodium bicarbonate, thus: $CO_2 + H_2O + Na_2CO_3 = 2\ Na\ H\ CO_3$. In this manner the carbon dioxide, which is separated in the liver by the process of oxidation, is carried from the seat of its production on to the lungs, which are the chief organs for eliminating the CO_2 from the system. From the capillary blood-vessels in the lungs the carbon dioxide and water are discharged into the air passages, and the sodium bicarbonate is transformed to a carbonate and returns again to the glandular organs, in this case to the liver for another charge of CO_2 and H_2O.

At the same time that these changes are being effected in the C H O compounds, some of the active oxygen absorbed into the protoplasm of the hepatic cells is attacking the proteid elements which have been taken up from the passing blood. Thus an active oxidation or splitting-up of the atoms of the proteid substances is brought about in the epithelial cells of the liver. And it is out of

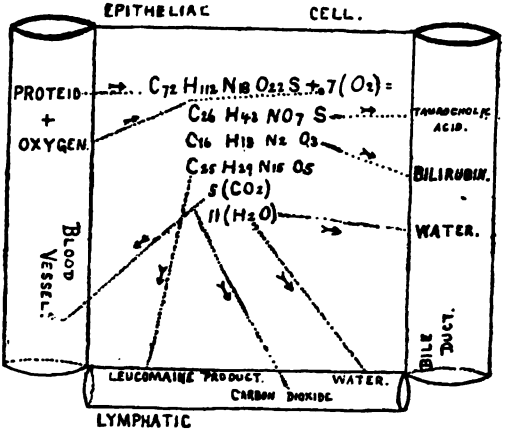

FIG. III—Proteid Oxidation. Schematic drawing illustrating the oxidation process in the protoplasm of the epithelial cell.

the proteid compounds that the bile acids and pigments are formed, thus: $C_{72}H_{112}N_{18}O_{22}S + 14O = C_{26}H_{43}NO_7S + C_{16}H_{18}N_2O_8 + C_{25}H_{29}N_{15}O_5 + 5\ CO_2 + 11\ H_2O$, as illustrated in Fig. III. When this oxidation of the proteid compounds in the hepatic cells is normally effected, the bile acids and pigments pass off by the bile-ducts, and the remaining $C_{25}H_{29}N_{15}O_5$ compound or leucomaine product passes into the lymphatic channels and on into the blood, and, after a time, reaches the renal organs. In the kidney this leucomaine product, which was developed in the liver as a side product at the time of the formation of the chemical constituents of the bile, is drawn into the renal cells together with some active oxygen. The oxygen at this point of the animal economy attacks this leucomaine product and causes it to be split up into the final katabolic bodies, such as urea, uric acid, kreatinin, carbon dioxide and water, thus: $C_{25}H_{29}N_{15}O_5 + 28\ O = 4\ CH_4N_2O + C_5H_4N_4O_3 + C_4H_7N_3O + 12\ CO_2 + H_2O$.

In this manner a logical chemical explanation is found for the development of all the constituents of the bile. A few grammes of the total intake of proteid elements are all that is required to furnish the quantity of nitrogen eliminated through the bile. The balance of the proteid substance taken into the animal economy daily is oxidized in the other glandular organs of the body, there forming the katabolic bodies of the various secretions. The major part of the work of oxidizing the proteid bodies into their final products is accomplished in the epithelial cells of the uriniferous tubules.

For a complete determination, however, of the total output of nitrogen, cognizance must be taken of the nitrogen contained in the ferment bodies, of that contained in the desquamating epithelial cells of the mucous membranes and integument, and of that in the hair and nails. In fact, every possible avenue for the escape of nitrogen must be sought out and its output of nitrogen carefully estimated.

When all this is allowed for, it is found necessary to introduce daily into the circulating medium of the body, the blood, 130.32 grammes of proteid elements to furnish the requisite quantity to form all these nitrogenous katabolic bodies known to be eliminated from the system.

During this process of proteid oxidation in the hepatic cells to form the active constituents of the bile, the resulting leucomaine may not be so perfectly formed as is the case in the absolutely normal state of the system. Under these circumstances, glycogen, cholesterin are found among the immediate products of proteid oxidation.

Glycogen has been for many years considered as a product of glucose transformation, but the weight of evidence tends to prove that this theory is based upon unsound chemical data, and that the true source of the glycogen is the result of an oxidation or splitting-up of the proteid elements in the hepatic cells, glycogen being one of the intermediate or by-products of the proteid elements, and not a derivative of the carbohydrate series. In this manner the formation of the active chemical constituents of the hepatic secretion, the bile, is explainable upon sound chemical laws. The reason why a few grammes of the proteid elements go to one organ and are there split up or oxidized to form the chemical constituents of its secretion, and a few to another, and so on through the whole list, is just as undefinable as life itself. But that such is the case is proved by the output of atomic elements exactly balancing the intake.

The nerves which are distributed to the liver are quite small and very difficult to trace with certainty. They are derived chiefly from the solar plexus, the pneumogastric, phrenic, and sphlanchnic nerves. By some investigators it is claimed that a small filament of nerve tissue is in direct communication with each mass of protoplasm which constitutes a liver cell. If the nerves do not actually enter the cell mass, it is highly probable that they are brought

into such close relation with the protoplasmic elements that they are influenced directly by the heat produced by the oxidation which is constantly taking place in the substance of the epithelial cells of the liver. By this close relation of the nerve filaments to the cells, a peripheral influence in the form of heat generated by the oxidation of the carbohydrate compounds and the proteid elements, which is constantly taking place in the epithelial cells of the hepatic lobules, is brought to bear constantly upon the nerve endings in the liver tissue. This causes a reflex or centripetal impulse to be carried more or less continuously up to the spinal and cerebral centres. Having reached the ganglion cells of the cerebro-spinal axis, the incoming impulses are transferred by the receiving ganglion cells to the discharging cells, from which they are sent out to the periphery again to act either as innervating or inhibitory impulses.

Thus the oxidation processes become the prime factor in maintaining nerve action and are brought into direct relation to the nervous system. It is by this constant heat production in the glandular organs of the body that nerve action is set in motion and maintained in action. Through this reflected nerve impulse from the periphery to the cerebral nervous system and from the center back to all parts of the animal economy, a uniform and harmonious balance of activity is developed and sustained throughout the whole system. All nerve action, therefore, takes its origin in the oxidation processes which are constantly occurring in the epithelial cells which constitute the coating of the body.

These surface cells, when bunched together, as occurs in the liver, salivary, pancreatic, and renal glands, are found to be the chief oxidizing zones of the body. Comparatively little oxidation occurs in the epithelial cells forming the integumental covering of the body. The same may be said of the epithelial cells of the alimentary

and genito-urinary tracts. But in the lungs the epithelial cells lining the deeper air-passages are the seat of quite as marked oxidation changes as are those of the larger glandular organs already mentioned. In fact, all the fat appears to be oxidized at this point; for if more fat is taken into the blood than can be attacked by these cells and their contained oxygen when first it reaches the lungs, the fat makes the complete circuit of the vascular system, and, when it comes around the second or third time, finds sufficient oxygen for its complete disintegration in the pulmonary tissue. The fat oxidized in the lungs produces heat, which is applied directly to the peripheral endings of the great pneumogastric nerve in the pulmonary tissue. This impulse is reflected along the centripetal nerves to the cerebral ganglion cells, and from there again sent out as described in connection with the nerves of the liver.

With an accurate knowledge of the histological construction of the liver, and a clear conception of the physiological functions of the hepatic gland, it becomes comparatively easy to explain how a form of jaundice can be produced by changes which are exclusively within the acini. Cases in which there is absolutely no mechanical obstruction to the bile-ducts between the surface of the lobules and the entrance of the common bile-duct into the duodenum, as shown when discussing the production of bile by the hepatic cells, so long as the liver maintains its normal physiological function, the glandular cells, which are located within the blind extremities of the bile-ducts, enlarge with each afflux of blood to the liver. When in this enlarged state the swollen cells partially occlude the primary radicles of the bile-ducts or capillaries. But as the vascular engorgement passes off, the liver cells draw back from the biliary outlet or mouth of the biliary radicles.

During the afflux of blood to the liver the carbohydrate

and proteid compounds contained in the blood of the entero-hepatic system of blood-vessels are drawn into the protoplasm of the hepatic cells from the passing blood stream, together with some active oxygen. Once within the substance of these cells, these chemical compounds are oxidized to produce heat and the constituents of the hepatic secretion. The bile thus formed passes from the protoplasm of the hepatic cells into the open biliary radicles, and from this its seat of production flows on to the bile capillaries and ducts to reach the lumen of the duodenum.

If now, for any reason, toxic agents are introduced into the animal economy, as, for instance, certain mineral poisons or some of the organic poisons, such as the micro-organisms and their products, the hepatic cells are called upon suddenly to aid in the destruction or elimination of these poisonous compounds from the animal organism. As a natural sequence, the hepatic cells are overworked, their nutritive activity is impaired, and soon they become swollen and undergo retrograde metamorphosis. Now, with each afflux of blood to the liver, the hepatic cells imbibe both nutritive pabulum and toxic compounds, and their protoplasm becomes larger and more abnormal in composition. In this abnormal condition, when the mass of protoplasm is forced into the primary biliary radicles, it fails to recede from the mouth of the bile radicle, as occurs in the normal state. As a result, the innermost end of the biliary capillary becomes partially or completely occluded, thus preventing, in part or totally, the escape of the bile by the natural biliary channels, as illustrated in Fig. IV.

As it has been proved conclusively that the blood-vessels cannot absorb the chemical constituents of the bile, and as no bile can escape by the bile-ducts, there is only one other channel by which the secretion of the hepatic cells can escape from the liver, and that is by the lymphatic channels. From the lymph space in which the

expanded internal and blind extremity of the bile-duct and its contained glandular cells are located, the bile flows along through the lymphatic channels to reach the

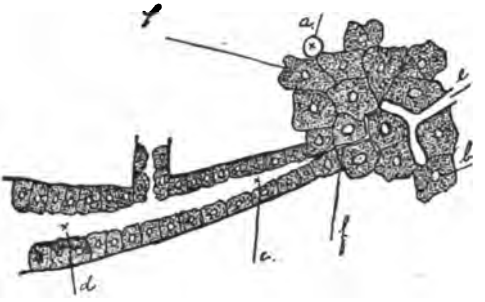

Fig. IV.—Abnormal liver, showing changes in the hepatic cells, and the relation of the cells, blood-vessels, lymphatics, and biliary radicles within the acinus. (x 850 and reduced by ¼).

a, intralobular blood-vessel; b, hepatic cells, swollen and abnormally granular; c, biliary passages beyond the neck, open and free; d, bile-duct, outside the lobule, free and open; e, lymphatic spaces, enlarged; f, biliary radicle containing the hepatic cells and neck of radicle occluded by the degenerated cells.

thoracic duct, on through it, finally reaching the blood stream at the origin of the left brachio-cephalic vein. From this point the constituents of the bile are carried by the blood stream to all parts of the body. A certain amount of the bile salts and pigments are eliminated from the system by the renal organs. But as the elimination of bile is not one of the natural functions of the kidneys, they are not able to cope completely with this extra task. Consequently, much of the pigment matter of the bile is deposited throughout the various tissues of the body. That it is the renal epithelium which is concerned chiefly in the elimination of the constituents of the bile, is evidenced both by clinical symptoms and the necropsy findings, the clinical evidence being the appearance of bile, and later albumin and casts in the urine. The latter are often typical epithelial casts, and each cell is granular and filled with the bile pigments. At the nec-

ropsy we find a marked metamorphosis of the epithelial cells, which, like the casts found during life, are granular and filled with bile pigment, while the other structures of the kidney are comparatively free from pigmentation.

A more chronic form of intralobular occlusion jaundice is produced by over-indulgence in the ordinary food stuffs, together with sedentary habits and imperfect oxidation. As a result of this condition, intestinal indigestion is developed, fermenting products of an abnormal kind are produced, and a more or less rapid introduction of these toxic products into the entero-hepatic blood is induced.

In these cases this slow infection induces a progressive augmentation in the amount of work to be accomplished by the hepatic cells, at the same time the nutrition standard is lowered. Consequently the protoplasm constituting the glandular cells undergoes a progressive degeneration, until some nervous disturbance, exposure, error in diet, or increased infection, often unnoticed by the patient, suddenly overtaxes these already overworked cells. They swell up a little more, and, as occurs in the acute form, the protoplasms become wedged into the mouths of the primary bile radicles, thus occluding completely the primary bile capillaries.

In some instances there is a complete occlusion of nearly all the intralobular biliary radicles at once. In these cases the symptoms will be rapidly developed and intense in character, with the typical clay-colored stools.

In other instances, when one radicle after another becomes occluded, the symptoms develop slowly, are less intense, often taking days or weeks before they become complete and marked.

In still another class of cases there is never a complete occlusion of all the intralobular biliary radicles, yet a sufficient number are occluded so that considerable bile is constantly flowing away from the liver by the lymphatic channels, sufficient to produce well-marked and persistent

symptoms of jaundice. From this class all grades are met with, down to slight cases of jaundice.

If the kidneys can eliminate the constituents of the bile as fast as they enter the blood stream, no symptoms of jaundice will occur. But just as soon as the eliminating capacity of the renal glands is exceeded, then the symptoms rapidly develop.

The symptoms of jaundice will not be taken up in detail, for they are familiar to all. Suffice it to say that they are mechanical from the deposition of the pigment, and toxic in nature from the general disturbance in glandular action and the retention within the system of various kinds and amounts of the products of imperfect oxidation.

The diagnosis is made by excluding all possible forms of mechanical occlusion of the bile-ducts between the surface of the lobule and the opening of the common bile-duct into the duodenum, and a careful remembrance of all the etiological factors which may act in exciting the intralobular form.

As to the prognosis, it may be said that in the acute forms of this variety of jaundice, if the hepatic cells of the liver were in a fairly good state of nutritive activity before the sudden introduction of the mineral or organic poison, the toxic agent will be rapidly destroyed or eliminated and the protoplasm of the hepatic cells will return to their normal composition and functional activity. If this is the case, all symptoms of jaundice will disappear and a complete recovery will result.

On the other hand, if the cells are in any degree impaired as to their nutrition and functional activity prior to the introduction of the poisonous compounds, the toxic agents will be slowly destroyed or eliminated, and the ultimate result will depend largely upon the treatment employed.

These cases may terminate in one of three ways: The patient may die shortly after the development of the

symptoms. A persistent lesion of the hepatic and renal cells may be the result, although the active symptoms have been dispersed. After the lapse of some time complete recovery may take place.

In all the chronic forms, the symptoms are much more prolonged than in the acute cases, which were in a fairly good physiological state previous to the development of the symptoms of jaundice.

In the confirmedly chronic cases, the prognosis depends largely upon the nutritive state of the hepatic cells and the character of the retrograde change that the protoplasm has undergone prior to the development of the jaundice and also upon the line of treatment employed.

This deflection of the bile from its natural channel and its introduction into the circulation causes an almost unlimited disturbance of the physiological economy. One abnormal condition follows in rapid succession upon another, so that when the patient comes to the physician for treatment it is with a very complex disturbance of the whole organism. The system is surcharged with various kinds and amounts of toxic substances; the intestinal canal is deprived of one of the most important digestive secretions, one which is essential for a perfect digestion of the food stuffs, and for the prevention of abnormal fermentation of the food products while they are being digested. Absence of the bile in the intestine further results in chronic constipation and the formation of toxic products in the alimentary canal, instead of an easily-absorbable nutritive pabulum only. These toxic principles are constantly passing into the entero-hepatic system, together with the nutritive pabulum, which is now decreased in amount as a result of the imperfect digestion.

Normally, the few toxic compounds that are produced and taken up from time to time are removed from the blood by the hepatic cells and cast off with the bile, but with the biliary radicles occluded, they pass on to the

general circulation. This, together with the decrease in the nutritive pabulum absorbed from the alimentary canal, naturally lowers the nutritive supply distributed throughout the whole system, and the constant impouring of toxines augments the excretory work. In consequence, all the tissues and glands of the body undergo retrograde changes, as compared with the normal physiological standard. These conditions decrease the possibility of repair being effected in the damaged hepatic cells by the inherent powers of the physiological economy.

The quality of the blood is deteriorated, the red blood-corpuscles lose a certain percentage of their hæmoglobin, even if there is not an absolute loss in the number of the corpuscles. Thus a truly anæmic condition of the system is progressively developed. The heart action becomes weak, which, together with the anæmia, still further lowers the oxygenating capacity of the system, until Nature, unaided, can no longer cope with the pathological problem, and death ensues, either from toxæmia or a form of slow starvation.

With this conception of the problem, the treatment of these cases becomes, in a measure, a simple task. The digestion must be improved and the abnormal fermentation processes in the intestines arrested. This cuts out one of the etiological factors and improves the supply of nutritive pabulum. At the same time, the rapidity of the circulation must be increased and the anæmia overcome; this accomplished, the nutritive pabulum is more perfectly distributed throughout the system and the oxygenating capacity of the animal economy augmented, and, as a natural sequence, both local and general assimilation and nutrition are improved.

While the digestion and assimilation are receiving attention, the swollen and degenerated hepatic cells that are occluding the biliary radicles must be decreased in size and stimulated directly to increased nutritive activity,

so that the occluded biliary radicles become again free and open and the bile that is formed in the hepatic cells can gain access to the alimentary canal through the bile-ducts.

The inherent nutritive state of the protoplasm constituting the hepatic cells must be restored to a normal state, that they may produce a good quality of bile before recovery can be called complete.

The excretory organs in general may need special attention to enable them to cope successfully with the increased work imposed upon them at a time when their nutritive supply is defective. This is especially so when there is a sudden exacerbation in the toxic poisoning of the system.

The cardinal principle governing the dietetic treatment of these cases is, that the different classes of food stuffs, such as the starches, sugars, fats, and proteids, must be furnished in such quantities only that the digestive functions will not be overtaxed, nor the oxygenating capacity of the system exceeded.

In some, the exclusive skimmed milk or buttermilk diet is the most valuable. This to be followed, as soon as fats can be assimilated, by unskimmed milk. Some will be able to take with the milk, beef tea and plain meat broths. Raw eggs are very serviceable in these conditions. As soon as the system will digest them, cooked eggs and meats should be given. Last of all, bread, in the form of a dry toast or plain wheat bread which is from twenty-four to forty-eight hours old.

One thing after another should be added until the following form of diet, which is one that yields the largest heat production, and at the same time develops the heat equally at its various generative foci in the body, as in the liver and lungs, and also supplies the full quota of constructive or proteid material, accomplishing all this with the smallest outlay of oxygen,—can be utilized:

198 44 gme.	(7.0 oz.)	of wheat bread.
102.05 "	(3.6 oz.)	of egg (equal to two eggs).
680.37 c. c.	(24.0 oz.)	of milk.
396.89 gme.	(14.0 oz.)	of meat, or its equivalent in fish.
1,377.75 "	(48.6 oz.)	

Of the above compounds, the breakfast should consist of:

56.69 gme.	(2.0 oz.)	of wheat bread.
102.05 gme.	(3.6 oz.)	of eggs, equal to two eggs.
226.79 gme.	(8.0 oz.)	of milk.
14.17 gme.	(0.5 oz.)	of butter.
399.70 "	(14.10 oz.)	

For the midday meal:

85.03 gme.	(3.0 oz.)	of wheat bread.
14.17 "	(0.5 oz.)	of butter.
198.44 "	(7.0 oz.)	of meat.
297.64 "	(10.5 oz.)	

For the evening meal:

56.69 gme.	(2.0 oz.)	of wheat bread.
14.17 "	(0.5 oz.)	of butter.
226.79 "	(8.0 oz.)	of milk.
198.44 "	(7.0 oz.)	of meat.
396.09 "	(17.5 oz.)	

The remaining 226.79 grammes (8.00 oz.) of milk can be taken during the evening from nine to eleven o'clock, depending upon the time of retiring; or it can be taken with one of the other meals if for any reason it is undesirable to take it separately during the hours just mentioned.

If milk in its natural state cannot be tolerated, some form of fermented milk, such as Kumyss or Matzoon, may be ingested. Beefsteak is taken as the standard meat to work from, as it is the most easily digested and assimilated. Under the heading meat are included all forms of fish, poultry, and game.

One thing that close experience teaches in the man-

agement of these and all forms of intestinal indigestion, is that every variety of fruit and vegetable should be excluded from the alimentary tract. The reason for this is that both the fruits and vegetables are prone to excite by their fermentation abnormal decomposition of the proteid compounds with the development of toxic substances, which is the very condition that needs to be prevented.

Even with the diet properly regulated, Nature, unaided, is unable to perfectly digest the food stuffs, and, therefore, they must be artificially digested, not outside the body, but within the alimentary canal. This is best accomplished by the administration of ox-gall and pancreatic extract before the meal, and hydrochloric acid alone or with a little pepsin after the meal.

The acid and the pepsin supply to the gastric cavity what Nature should, but in this abnormal state cannot, produce in sufficient quantity, so that the artificial addition of these two substances augments the proteid digestion in the stomach, even to a point above the normal standard. This decreases the amount of digestive work to be performed by the pancreatic and intestinal ferments. The artificial addition of the bile and pancreatic extract in a similar manner furnishes to the intestinal canal the needed digestants which Nature is unable to produce in sufficient quantity. The result is a more perfect intestinal digestion, with a decrease in the abnormal fermentation, which is followed by a larger supply of nutritive pabulum into the entero-hepatic circulation and a decrease in the toxines. In some instances, the addition of some intestinal antiseptic may be needed to arrest the abnormal fermentation and to prevent the development of toxines and auto-infection. With this, some laxative or cathartic principle must be used to keep the bowels open. From one to three movements should be secured daily. With the bowels free, the toxines will be carried out with the fæces,

and their liability to enter the blood stream is reduced to the minimum. The cathartics and antiseptics work best when combined with the bile and pancreatic extract, and should be administered before each meal.

The sluggish action of the heart and circulation is best overcome by the use of the muriate of caffeine and strychnine, the former of which also favorably influences the action of the renal glands.

The anæmia is best treated by the use of hæmogallol in from three- to six-grain doses.

Last, but not least, in the treatment, is that which directly influences the enlarged and degenerated cells of the liver which are occluding the biliary radicles. These are best reached by calomel in varying doses alone, or in combination with arsenic, bichloride of mercury, and ipecac. The three mineral compounds are taken up by the entero-hepatic circulation and are carried up to the liver. When these mineral compounds reach the intralobular capillaries, which lie in close relation to the hepatic cells, the protoplasm constituting these cells has the power to draw the little molecules of arsenic and mercury into its substance and discharge them into the bile-ducts with the bile. This act irritates the protoplasm and increases its functional activity to such an extent that they secrete the bile acids more rapidly than they can be converted into the neutral salts of the bile acids. Consequently, the bile acids flow on into the intestine, where they act as direct irritants to the nerve-endings in the coat of the intestine, thus exciting active peristalsis and catharsis, carrying with them the insoluble calomel, which is absolutely inert, so far as the intestine is concerned. The arsenic and the bichloride, being slightly soluble, are more powerfully irritating to the hepatic cells, and consequently greatly enhance the stimulating or irritating effects produced within the cell protoplasm.

By thus increasing the functional activity of the proto-

plasm at the same time that the etiological factors have, in a large measure, been removed, the digestion and assimilation in general improved, the pathological protoplasm in the hepatic cells gives place to that of a normal character, the bile radicles are opened, the bile flows from the liver by the natural biliary passages, and, after a time, a perfectly normal state of the system is re-established.

In some of the more chronic cases, the iodides are required to set the hepatic cells in motion, which act in a manner similar to the soluble mercury and arsenic—purely by their mechanical properties.

DISCUSSION.

DR. A. L. LOOMIS.—I hardly feel able to discuss Dr. Porter's paper along the line in which he has presented it. In a scholarly and clear manner he has laid before us the more advanced views now held regarding the histology and physiological functions of the hepatic lobules, and so far as they are concerned, I can see no reason to question his position. When we come to the general subject of jaundice we are entering upon one which has been discussed in very different ways and from very different standpoints, and with very different ideas in the minds of those who have taken part in the discussion. Even at the time when I first began to study medicine I was struck with the fact that the hæmatogenous form of jaundice could not be clearly explained. I was unable to understand it myself nor could I explain it to others. Some time ago I took the position that all forms of jaundice were obstructive. The only question in a given case is, Where does the obstruction lie? Of course, we all recognize the fact that the seat of the obstruction may be either in the liver or external to it. For some years past I have held the view that the only way in which we can explain those forms of jaundice which occur during the course of certain infectious diseases (the so-called hæmatogenous jaundice) is to accept the theory that the function of the liver cells is interfered with in some way by

the infection. I need not repeat to you the various forms of degeneration of liver cells which are the result of infection. As Dr. Porter has very satisfactorily explained, the cells themselves may become the obstructing cause.

As regards the secondary effects of hepatic obstruction, I believe that the nervous symptoms make their appearance before those due to derangement of the kidneys.

From the microscopical appearance of the liver tissues in cases of chronic jaundice I have been led to believe that whenever the jaundice occurs in connection with intra-hepatic changes, the primary change begins in the radicles, followed by secondary alterations in the connective tissue.

Dr. Porter's paper certainly opens to us some very interesting problems in regard to the management of these cases. I am fully in accord with all he has said concerning the diet as a most important element in the treatment, but I do not think he has laid sufficient stress upon oxygenation. Plenty of fresh air and sunlight are of the utmost importance. As regards diet, the quantity of food should be regulated as well as the quality. I am not in favor of giving these patients plain milk, preferring either kumyss or buttermilk. As regards climate, I think these patients do well in an inland climate, at a moderately high altitude, say 2,000 or 2,500 feet.

DR. A. H. SMITH.—In my experience I have never happened to meet with, or at least to recognize, any case in which the jaundice was due to intralobular occlusion. The clinical picture which Dr. Porter presents to us in his paper is familiar enough, but I do not see how it can be differentiated from those cases where the jaundice is due to an obstruction outside the lobules. It seems to me rather difficult to grasp the idea that the bile is formed in the hepatic cells, and at the same time that the jaundice is due to a disorganization of those cells; from this we would infer that the lesion producing such a form of jaundice is rather a serious one. In many instances, I think, nervous influences play a very important part in the production of jaundice. In no other way can we account for the sudden appearance of bile pigment in the skin after profound mental shock, or in certain emotional conditions. It certainly points to a marked nervous dis-

turbance. One explanation of the phenomenon has been that it is owing to vaso-motor influences, which produce a disturbance of equilibrium of the pressure in the blood-vessels and bile-ducts.

If the rules of diet, etc., which Dr. Porter laid down in his paper are also to be applied to cases which we at present call catarrhal jaundice, I have no criticism to make, excepting to express the opinion that in mild cases such rigid treatment is hardly called for.

DR. R. C. M. PAGE.—The two forms of jaundice usually described are the hepatogenous, which originates in the liver, and the hæmatogenous, which is claimed to be due to a disintegration of the red corpuscles, and a conversion of their coloring matter into bile pigment. Bile is formed in the liver. If it has free outlet along the bile-ducts and through the intestines, there is no jaundice, but if, from any cause, its flow from the liver is obstructed, it is absorbed by the lymphatics, and passing along the thoracic duct into the general circulation, produces jaundice of most of the tissues and fluids of the body. This form of jaundice is known as hepatogenous or obstructive, in contradistinction to the hæmatogenous or non-obstructive. In many cases the retention of the bile in the liver is very easily explained: It may be due to catarrh of the bile-ducts, to gall-stones, or to pressure from without, produced by tumors, etc. In other cases, however, the cause of the obstruction is not so apparent. It should be borne in mind, however, that the expulsive force of the bile from the liver is feeble, and that it requires but a very small amount of resistance to produce retention. In cirrhosis, jaundice is the exception rather than the rule, and then it is thought to be due to some complications, as catarrh of the ducts or to the presence of gall-stones. In some grave forms of disease, especially when attended with cachexia, there is a certain amount of discoloration of the skin, but hardly sufficient to be called jaundice. True jaundice depends on the coloring matter of the bile, and it is probably due to hepatic obstruction in all cases, although the cause of the obstruction is not always apparent. If this is so, then there exists in reality no such thing as hæmatogenous jaundice.

DR. PORTER (closing the discussion).—I am glad to

hear Dr. Loomis and Dr. Page express it as their conviction that all forms of jaundice are probably obstructive in character. The so-called hæmatogenous form has never been clear to my mind until I worked out the explanation as given in this paper, which I think is a logical one. The trouble in these cases is in the hepatic cells, and not due to blood changes.

I was glad to hear Dr. Loomis emphasize the importance of oxidation in the treatment of these patients. They are in a condition of progressive anæmia, and require the largest possible amount of oxygen. The explanation given in my paper also enables us to understand those cases where we have chronic jaundice, lasting for months or years. Some of the hepatic cells undergo this degeneration and the bile enters the general circulation and gives rise to chronic jaundice. Of course, the treatment outlined in my paper was not intended to be carried out in full in the milder cases. The activity of the treatment must be governed by the severity of the pathological lesion.

Stated Meeting, November 15th, 1894.

THE TREATMENT OF INOPERABLE MALIGNANT TUMORS WITH THE TOXINS OF ERYSIPELAS AND BACILLUS PRODIGIOSUS.

By WILLIAM B. COLEY, M. D.

I am conscious that the treatment of inoperable tumors is a very trite subject, yet in consideration of the fact that practically no advance has been made in this field since the disease was first known, I am sure I need offer no apology if I can show that there has been even a single step forward. No one can blame the profession for sound conservatism, or even scepticism, in regard to anything that is claimed to cure inoperable malignant tumors. Past experience fully justifies its attitude, and I believe it should not accept a single statement until it has stood the rigid tests of scientific demonstration. By these tests alone I desire you to judge the cases I have already reported, as well as those I have the pleasure of presenting to-night. My results in thirty-five cases of inoperable tumors treated with the toxins during the past three years, were reported in detail at the last meeting of the American Surgical Association[1] at Washington, May 31st, 1894, and will be only briefly referred to here.

Four very remarkable successes since then, and the additional experience gained from the observation of twenty-four new cases, are, I think, sufficient reasons for bringing this subject again to your attention.

Up to last May my cases were treated with the filtered

[1] American Journal of the Medical Sciences, July, 1894.

toxins, prepared by growing the erysipelas streptococci in bouillon three weeks, then filtering through porcelain, and preserving by adding thymol. The toxins of bacillus prodigiosus were prepared in the same way, and the two were mixed at the time of using. In some of the cases the two germs were grown together in the same flasks, and then filtered. This method of preparation failed to utilize whatever of value exists in the bodies of the germs themselves, and I decided to try preparing the toxins by subjecting the cultures to the lowest temperature that would suffice to destroy the germs. This I found by experiment to be about 58° C. This preparation has been used exclusively in the series of twenty-four cases which I report to-night, and it is, I think, much more efficacious than the filtered toxins.

The following is a brief history of the more important cases treated since June 1st, 1894, and not yet reported:

CASE I.—Female, twenty years of age. Recurrent spindle-celled sarcoma of palm of hand; six operations. This case appears in the tables of my previous paper (*loc. cit.*) as Case VI, and is merely mentioned as having shown marked improvement, and as being still "under treatment." The subsequent history makes the case worthy of a fuller report.

The first operation was performed in the fall of 1888. The tumor did not recur until the summer of 1891. A second operation was performed and recurrence again followed; the third, fourth, and fifth operations were performed, recurrence each time occurring after shorter intervals.

The fifth operation was performed in January, 1893, by Dr. Samuel Lloyd, of New York, and recurrence followed within three weeks. I saw the patient, in consultation with Dr. Lloyd, in February, 1893, and at that time a tumor, the size of half a hen's egg, was seen in the palm of the right hand at the site of the old cicatrices. It was a typical sarcoma clinically, and the diagnosis had been repeatedly confirmed by microscopical examination. Am-

putation of the forearm had been very strongly urged, but the patient refused consent to such operation. The axillary glands were slightly enlarged; her general condition was only fair. Injections with the mixed toxins were begun under my direction, and often by myself personally, and the treatment was continued by Dr. Lloyd, with longer or shorter intervals, during the whole spring. The tumor decreased some in size and failed to grow when the injections were stopped for a short time. She had little treatment during the summer, and in October, 1893, the axillary glands were very much enlarged. The tumor in the hand was also beginning to increase in size. Dr. Lloyd operated upon the axilla and removed several glands, some of which were as large as large olives, and typically sarcomatous. The tumor of the hand was then treated with the injections of the mixed toxins.

The patient was unable to remain in a hospital, and the injections were given at Dr. Lloyd's office. This made it impossible to push the injections to the desired limit, and made the treatment otherwise unsatisfactory. Whenever the injections were given regularly and in fairly large doses the tumor remained in control, and furthermore decreased in size.

In June, 1894, she was admitted to my service at the New York Cancer Hospital, and the injections were pushed to their limit for about three weeks. The tumor softened and also decreased some in size. As the patient's general condition had been running down for several months, I thought it wise to give ether and scrape out the more or less necrotic sarcomatous tissue. It was found to extend very deeply, entirely surrounding the palmar tendons of the middle, ring, and little fingers, and infiltrating between the metacarpal bones so as almost to appear on the dorsal side. It was removed as well as possible with a Volkmann spoon and scissors, and the extensive wound allowed to heal by granulation; no further injections were given. She quickly began to improve in health, and has now regained her normal weight and strength. She has gained twenty pounds since July. No trace of any trouble can be detected either in the hand or axilla. This case is important, as it shows that a rapidly growing sarcoma was held in check one year and a half by the toxins, even when given

under unfavorable conditions, and also that the malignancy of the disease was probably destroyed.[1]

CASE II.—This case I owe to the courtesy of Dr. George F. Shrady, who kindly asked me to begin the treatment at St. Francis Hospital. Bernard M——, aged twenty-three, German, printer by occupation. His family history is good, and up to one year ago he had enjoyed perfect health. Ten years ago he fell from a fourth-story window, but was supposed to have suffered no permanent injury. Three years ago he fell in the street, striking upon his back. He, however, noticed no trouble from either of these injuries, until one year ago a tumor appeared in his back, in the region of the right buttock. There was little pain, and his attention was first called to it by the fact that he was unable to button his trousers. The tumor was hard, apparently rising from the ilium, and gradually extended until it had involved nearly the entire right half of the ilium. In February, 1894, it had reached the size of a child's head, and extended from the middle of the sacrum behind nearly to the edge of the rectus muscle in front, filling up a large portion of the right iliac fossa. The tumor was firm in consistence, the skin over it normal, and not adherent. The right inguinal glands were very much enlarged. The patient was considerably emaciated, and was losing flesh and strength rapidly.

He was shown at the New York Academy of Medicine by Dr. Kammerer, in February, 1894, before the Surgical Section, and the question as to the advisability of subjecting him to the treatment with erysipelas and prodigiosus toxins was discussed. The weight of opinion was that the tumor was too large and the case too far advanced to offer even a hope of success. It was thought best, however, to try the injections for a brief time, and the treatment was begun at St. Francis Hospital, in March, 1894. Injections were continued at intervals of twenty-four to forty-eight hours for a period of one month. The tumor very quickly began to break down, and fluctuation appeared over a large area. Free incision and irrigation were employed, and the tumor continued to

[1]The case remains in perfect health January 1, 1895, nearly six months since cessation of treatment.

break down and slough out in large masses for a number of weeks after the injections were stopped.

The patient was considerably depressed both by the treatment and by the sloughing process, and for some weeks his condition was exceedingly critical. His temperature ran very high for a month after the injections had been stopped, due to the absorption of the broken-down tissue. As the discharge began to diminish, his condition improved, and by June he was able to be up and about the ward. Since that time he has steadily improved, regaining his flesh and strength with remarkable rapidity, and on September 12th, 1894, physical examination was as follows: In right lumbar region, one inch below the crest of the ilium, is a cicatrix three inches long with a small sinus at either end. Two inches below this cicatrix is another small sinus; four inches further down, a third. No dead bone can be detected with a probe, and there is only a slight discharge from the sinus. The inguinal glands on the right side are slightly enlarged, but steadily diminishing in size. There remains no trace of the large tumor already described at the beginning of the treatment, and the general condition of the patient has nearly returned to normal. He now weighs (November 15th), one hundred and forty-eight pounds, a gain of twenty-nine pounds since August, and fully forty pounds since June. He has resumed his occupation as a printer.

I will say that no microscopic section was examined before treatment was begun, but the clinical history and physical examination, verified by a large number of surgeons of the Surgical Section at the Academy, left no doubt as to the diagnosis. It was an osteo-chondro sarcoma, as several hard, cartilaginous masses sloughed out, examination of which showed them to be chondroma. The majority of the tumor was sarcomatous tissue.[1]

CASE III. *Spindle-celled sarcoma of scapula with an extensive involvement of chest wall.*—Sarah C——, aged sixteen, was admitted to the New York Cancer Hospital June 20th, 1894. There was no history of malignant disease in the family, and her personal history was good up

[1]Since the paper was read, a portion of the thickened ilium at site of tumor was removed, December 1st, 1894, for examination, and no trace of sarcoma could be found on microscopical examination.

to four months previous to admission. At that time she first noticed a small tumor in her back, over the left scapula. The tumor rapidly increased in size, and gradually extended, both in front and behind, until it had involved the larger portion of the left thoracic wall. It was associated with constantly increasing pain and interference with the movements of her arm.

Physical examination, June 25th, 1894, showed the following condition: There was a hard fixed mass involving almost the entire region of the left thorax behind, extending from the clavicle to the lowest rib, vertical measurement being thirteen inches. Horizontally, it extended from the median line behind to the middle of the sternum in front, and from one inch below the clavicle seven inches downward, involving a large portion of the region of left breast. The entire tumor was firmly fixed to the chest wall, but the overlying skin was normal in appearance, and not adherent. The left arm could not be raised to a right angle, and movements forward and backward were correspondingly limited. A portion of the tumor was removed, and microscopical examination proved it to be a spindle-celled sarcoma. In consideration of the rapidity of the growth and enormous extent involved, I did not believe even temporary arrest possible; nevertheless, I decided to try the injections for two or three weeks and watch the effect. To my astonishment, improvement was immediate and very rapid. Within three weeks the arm could be raised to a vertical position, and within one month the growth in front had nearly disappeared. The injections were continued at intervals of from twenty-four to forty-eight hours, and the reactions were never severe enough to keep the patient in bed more than a few hours after the injection.

The improvement continued without any interruption until the latter part of October, when no trace of the tumor could be found either in front or behind. The treatment has been discontinued since that time, and the patient is presented this evening without the slightest suspicion of the sarcoma or even any induration visible. She has regained her normal weight and her usual health and strength.

This case is to my mind the most brilliant of the entire series, and must be regarded as the crowning triumph of

the toxin treatment. Here we have a tumor, the great malignancy of which was shown by the rapidity of its growth and the extensive involvement that had occurred within four months, and the diagnosis of spindle-celled sarcoma established by the microscopic examination of a skilled pathologist,[1] entirely disappearing within three and a half months from the beginning of the treatment with the mixed toxins. There are some points of unusual interest in this case:

1. All of the injections were made in one portion of the tumor, over the lower part of the scapula, and within a radius of two inches.

2. There was no breaking down, the tumor disappearing entirely by absorption.

3. Only a single preparation of the toxins was used throughout, viz., mixed cultures of erysipelas streptococci and bacillus prodigiosus, grown together in bouillon and sterilized by subjecting them to a temperature of 58° C.

CASE IV. *Probable sarcoma of sternum.*—The patient, aged thirty-nine, female, was first seen by me in consultation with Dr. Farquhar Ferguson, August 21st, 1894. Two of her sisters had suffered from cancer, but she herself had been well up to four months before consulting me. At that time she noticed a small swelling over the upper portion of the sternum. It steadily increased in size, was but slightly painful, and seemed fixed to the sternum. Her general health had become considerably impaired and she had lost weight. Physical examination showed the upper third of the sternum occupied by a tumor three inches in diameter, symmetrical in outline, and firmly connected with the sternum itself. In consistence it was about the same as an ordinary sarcoma when the cellular structure predominates. A few enlarged glands were present along the border of the left pectoral muscle.[2] The injections were begun August 23d, 1894, and severe reactions followed the administration of very small doses. The injections were repeated every other day, and at the

[1] Dr. G. F. Brooks, Pathologist to Post-Graduate Medical School and Hospital.

[2] Material drawn with aspirating needle was examined by Dr. Ferguson and he believed it to be sarcoma.

end of the first week most of the tumor was soft and fluctuating. A small incision was made under cocaine and the broken-down material allowed to escape. The injections were continued in the portions of the tumor still hard for about four weeks longer. At the end of that time it had apparently disappeared, there only remaining two small sinuses at the points where incision had been made to permit the degenerated tumor tissue to escape.

Her general health has much improved, and although the case is too recent to speak of it as a permanent cure, the results in my earlier cases make it not unreasonable to hope for a cure in this case.[1]

Carcinoma.—Of the cases of carcinoma I have only time to mention in detail a single case.

Mrs. W——, aged thirty-four, with a good family and previous personal history, came to the Methodist Episcopal Hospital in July, 1894, with a very rapidly growing epithelioma of the lower jaw, involving the floor of the mouth, under portion of tongue, and all of the soft tissues of the chin. There was an area the size of a silver half-dollar, the site of a typical epitheliomatous ulcer. The tumor had followed quickly an injury to the chin, and this fact, in connection with the age of the patient, would have pointed to a diagnosis of sarcoma, had not an examination showed it to be epithelioma.

She was kindly referred to me by Dr. George R. Fowler, and treatment was begun early in July; from three to seven minims of the mixed toxins were injected into soft parts of chin which were involved in the disease. Within two weeks the ulcer had nearly healed, and within three weeks it had entirely healed and the parts almost regained their normal appearance. The floor of the mouth also showed a striking improvement. This improvement unfor-

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[1] A tumor in the left lumbar region broke down under the injections in sternal tumor and discharged material that could not be differentiated from that coming from sternal tumor.

Later Note.—January 15th, 1895. A recent examination of scrapings from sinuses in sternum and back, by Professor E. K. Dunham, shows that both tumors were probably of tubercular origin. The effect of the toxins is in harmony with the well-known effect of actual erysipelas upon tubercular lesions of skin and glands referred to by Mauriac.

tunately did not continue more than a few weeks, and then a change for the worse appeared, and the injections no longer seemed to control the disease.

A number of other cases of carcinoma, including epithelium, showed striking but only temporary improvement. I have seen very great œdema of the arm, caused by recurrent cancer of the breast, disappear with astonishing rapidity after two or three injections.

My feeling as regards the treatment of carcinoma with the toxins has, up to the present time, been very conservative, and until further experiments have been made I should not advocate its general adoption.

The increasing number of well-authenticated cases of carcinoma that have been permanently cured by attacks of accidental erysipelas, as well as the undoubted antagonistic action I have observed in my own cases treated with the toxins, should make us extremely hopeful that, in the near future, some of the success that has been achieved in sarcoma may be extended to carcinoma.

This is not the time nor place to attempt an explanation of this curative action of these germs. It would bring up the whole discussion of the etiology of cancer, a question surrounded by almost innumerable obstacles, and not yet settled to the satisfaction of the majority of the profession. Personally, I believe the evidence at present strongly in favor of the micro-parasitic theory of the origin of both sarcoma and carcinoma. Accepting this theory, the explanation of the action of toxins on malignant tumors is very easy; on any other theory I believe an explanation impossible.

Results with others who have tried toxins in the way I have recommended, and using the same preparations.—At the American Surgical Association at Washington last May, in the course of the discussion, Dr. Keen stated that he had had no beneficial results in several cases that he had treated with the toxins. In my reply I stated that, up

to that time, I had had the greatest difficulty in getting a stable preparation and one that could be relied upon for any length of time. The results that I reported at the time only followed persevering and often prolonged efforts, using many cultures differing greatly in value. It should be remembered that up to that time I had not recommended the general use of the toxins, and only at the request of a few surgeons whom I personally knew, was any sent for trial. It is clearly unfair to allow the results of a few such imperfect experiments to stand as a test of the value of the treatment.

Fortunately, since that time, with the help of Mr. B. H. Buxton, who has during the past year devoted a large portion of his time to investigating this subject, and aiding me in perfecting the preparation, it has been found possible to obtain a preparation of the mixed toxins sufficiently stable to render accurate dosage possible. It is much more effective than any I had hitherto used, due to the increasing virulence of the streptococcus caused by passing it through a series of rabbits.

The method of preparation I will refer to more in detail later, but at present I wish to refer to a few cases to prove that like good results can be obtained from the treatment in other hands than those of its author.

Dr. R. Tilly of Chicago, in a letter dated August 16th, describes a case of sarcoma of the orbit, which he considered beyond operation, and of absolutely unfavorable prognosis. He tried at first the filtered toxins, with little or no benefit. He then used the unfiltered, including the bodies of streptococcus and B. prodigiosus, killed by temperature 58° C. The first three doses had no effect; the fourth gave a very severe chill and a temperature of 106° F. Within twenty-four hours the temperature was 97° F. The injections were continued about every other day for two or three weeks without much rise of temperature.

The tumor steadily decreased, and on August 16th,

1894, it had entirely disappeared. I have not heard of the subsequent history of the case.

Dr. J. Alexander Moore, of Helena, Mont., has used the toxins in four cases. He obtained the preparations from me, and used them as nearly as possible in the way I have advised. His first case was a scirrhous cancer of breast, recurrent. The left lung and liver were involved. He began injections January, 1894. He states that the disease was undoubtedly restrained locally for some time, but owing to the advanced stage and generalization the treatment was discontinued.

CASE II.—Epithelioma involving entire lower lip, two years' history. Growth ulcerating. Injections for above three weeks failed to produce any decrease in size.

CASE III.—January 15th, 1894. The patient, aged thirty-one, female. Recurrent, small, round-celled sarcomata of supra-clavicular region. Operation had been done May, 1893. Recurrence followed in two months. Injections with toxins of erysipelas and prodigiosus were begun January 15th, 1894, and continued every two or three days until February 15th; at that time the original tumors, several in number, had entirely disappeared, but other new ones appeared.

The patient lived at a distance from town, and the treatment could not be given with regularity. On September 15, 1894, there were five small nodules to be felt at site of former tumors. Dr. Moore writes: "The injections, when prosecuted with vigor, have never failed in this case to reduce or obliterate the tumors." When the nature of these tumors—small round-celled sarcoma, the most malignant type—is considered, the efficacy of the toxins cannot be doubted.

CASE IV.—Male, aged thirty-one, tumor, size of a goose egg, attached to right mastoid bone, and not adherent to skin. The lymphatics were enlarged and indurated. The growth had been noticed two months. Treatment was begun April 4th, 1894; very good reactions followed. There was some breaking down of the tumor. August 20th the tumor had entirely disappeared. Two

small lymphatics could still be felt just above the clavicle, but these were gradually becoming smaller. It is true, no microscopic examination was made in this case, but the case had been examined by other physicians of skill and experience.

Dr. Moore writes: "I am quite satisfied as to the effect of inoculation with the toxic products of erysipelas and B. prodigiosus in sarcoma, but only reasonably so in carcinoma."

Dr. George M. Kreider, of Springfield, Ill., surgeon to St. John's Hospital, writes me that he has used the toxins in three cases. The preparation used was the unfiltered, sterilized by 58° C., prepared by Mr. Buxton, the same as used by myself in the case reported.

One case was an inoperable sarcoma of the ilium of unusual size, involving not only the ilium but surrounding tissues. He writes that the result was very striking, but owing to the prostration of the patient he was obliged to stop the treatment. "Two hundred grammes (6 1-4 ounces) sloughed out, and it would seem certain that, had treatment been undertaken sooner, he would have recovered."

"The other cases of cancer have made gratifying progress, notably one case of uterine cancer. She was bedridden because of the swelling of the right leg and foot. The swelling disappeared entirely in three weeks, and has not returned, although the patient is on her feet continually. The pains and bleeding have also disappeared."

Dr. Thomas F. Rumbold, of San Francisco, has tried the toxins in a patient suffering from recurrent round-celled sarcoma, with the following result: In June, 1892, a tumor the size of a hickory-nut was noticed in the right breast. On July 7th, 1892, it was removed and pronounced non-malignant, but within a few weeks it returned; a second operation was performed, taking off most of the breast. Within six months third and fourth operations were performed, removing the whole of the

breast but not the axillary glands. In July, 1893, the tumor again recurred. A very thorough operation was then made by Drs. Lane and Plummer, of San Francisco, and the axillary glands, which were then involved, were removed. A microscopic examination showed the tumor to be round-celled sarcoma. Since then three other operations, making eight in all, were performed. The sarcoma again recurred and was growing rapidly, when early in August, 1894, Dr. Rumbold wrote me, requesting me to send him some of the toxins. The first injection was made August 14th, 1894.

In a letter dated September 12th he says: "I am very glad to inform you that the case has progressed very favorably; in fact, the treatment has been a marvel in all respects to every one who has seen the case. She is visited twice a week by a number of the most prominent physicians, who are greatly interested in the case and treatment. I do not think there is one eighth of the tumor left, and I believe she would recover entirely without another injection; but, to make matters sure, I will continue the injections, as suggested in your letter."

A later letter states that the tumor had entirely disappeared. The patient was considerably weakened by the treatment and was suffering from persistent nausea.

Time will permit me to refer to only one other case outside of those of my own personal cases; but this case is too important to leave unnoticed. As the case was reported in full at the last meeting[1] of the Laryngological Section of the Academy, I need only give a very brief summary of the facts:

The patient was a boy, aged sixteen, with a very large, spindle-celled sarcoma of the palate and pharynx. He was a patient of Dr. Walter B. Johnson, surgeon to Paterson General Hospital, of Paterson, N. J., and I was asked to see the case in consultation, in October, 1893.

[1] Medical Record, 1894.—XLVI—616.

The physical examination at that time, made by Dr. Johnson, showed a tumor involving the entire soft palate, pillars of the fauces, region of the tonsils, and extending forward over the hard palate to within a half inch of the incisor teeth, backward and downward, involving a portion of the pharyngeal wall, the whole base of the tongue, the epiglottis, and upper part of the larynx, but not extending to the true vocal cords. The infected parts were thoroughly impregnated with sarcomatous deposit, and the soft palate was increased to about three times its normal thickness. The growth consisted of cauliflower-like granulations, varying in size from a kernel of rice to a pea. The uvula was entirely destroyed; the cervical glands were enlarged. The patient had lost considerable flesh and strength, but was not cachectic. His weight was 86 pounds. He had difficulty in deglutition, and could not breathe through his nose. A microscopical examination made by Dr. James W. Williams showed the growth to be spindle-celled sarcoma. The tumor was so extensive that I expressed very little hope of any permanent benefit being derived from the toxins, but advised giving the treatment a short trial.

Dr. Johnson began October 31st, 1893, and continued the injections, with occasional intervals, for seven months. The reaction temperature following the injection varied between 99° and 103° F. The preparation used was the filtered toxins, grown separately and kept in separate bottles, mixing them at the time of injection. I had not then begun growing them together.

The result of the treatment was a constant, slow improvement, some of the sarcomatous tissue undergoing necrobiosis, some disappearing by absorption. In October, 1894, the patient weighed 107 pounds. He had had no treatment for three months, and the tumor had apparently entirely disappeared. (Two days ago, Dr. Johnson wrote that there was one point in the mouth that looked slightly suspicious, and he decided to resume treatment for a short time. This same thing occurred once before, and quickly disappeared when the injections were begun.)

Since the reading of the paper, another successful case has been reported to me by letter, and its great importance warrants inserting it here. The case was a patient of Dr.

Judson C. Smith, of New York, director of the laboratory of the Post-Graduate Medical School and Hospital, and was treated by him personally. He writes: "You may refer to my case of sarcoma treated with the toxins as follows:

"Mrs. W——, aged twenty-eight, good family and personal history. Tumor of right side of neck about the size of an orange; had been growing one year. September 1st, when she came under my care, she was extremely emaciated, anæmic, and scarcely able to leave her bed. The tumor was beginning to break down, and the patient was septic, having a temperature of 103° F. She also had dropsy of lower limbs.

"On September 11th, 1894, I made an incision under ether, and scraped out a large mass of the tumor, but owing to the patient's poor condition, no attempt was made to remove the whole mass, which was deeply seated, extending beneath the clavicle. The wound was packed with gauze, and four days later I began the injection of erysipelas and prodigiosus toxins. These were continued eight weeks. She has no sign of tumor now, has gained twenty-five pounds in weight, and appears perfectly well. Microscopic examination proves the growth to be small, round-celled sarcoma."

These results, in the hands of independent observers, are sufficient, I think, to confirm my own conclusions as to the value of the toxins in sarcoma.[1]

As to the question whether or not the toxins should be tried in all cases of inoperable sarcoma, there are many things to be considered. In the hands of men of experience, and very carefully administered, I believe the treatment to be attended with but little, if any, risk. My results show what may be accomplished in apparently the most hopeless cases; hence, the importance, if possible, of giving such cases the benefit of a trial. If improvement is likely to occur, my experience has been,

[1] Since the paper went to press, several additional successes in sarcoma in the hands of other surgeons have been reported to me by letter.

that some evidence will present itself within two weeks after beginning the injections. It must be remembered that we have to deal with a very powerful, and as yet not thoroughly understood, agent, and it should be used with the greatest caution. Individuals vary greatly in their susceptibility to its action, and the minimum dose should be used at first. I have myself never had a death from it, though in one case I think the end was probably hastened. This was an enormous sarcoma of the scapula, involving clavicle, chest wall, and a large portion of the humerus. The patient was very weak and emaciated, and the treatment should never have been attempted in a case so far gone. Only a single dose, and a very small one (two minims) was given; the patient gradually grew weaker, and died four days later.

I will merely refer to another field for the use of the toxins, and one that seems to me likely to be very important, viz.: After primary operation for sarcoma, before recurrence has been allowed time to take place, the reasons for using the treatment in such cases are theoretically strong. If, as we have demonstrated, the toxins are able to destroy a large recurrent tumor, there is reason to believe their action would be more certain and effective in destroying the few and invisible germs of the disease left behind after operation. Since at least seventy-five per cent. of the cases do recur after primary operation, the importance of this field is apparent.

Method of Preparation of the Toxins.—To make the toxins of erysipelas and prodigiosus, ordinary peptonized bouillon is put into small flasks, containing 50 to 100 c. c., which, after proper sterilization, are inoculated with the streptococci of erysipelas and allowed to grow for three weeks at a temperature of 30° to 35° C. The flasks are then inoculated with B. prodigiosus, and the cultures allowed to grow for another ten or twelve days at room temperature. At the end of that time, after being well

shaken up, the cultures are poured into sterilized glass-stoppered one-half ounce bottles, and heated to a temperature of 50° to 60° C. for an hour; sufficient to render them perfectly sterile. After cooling, a little powdered thymol is added as a preservative, and the toxins are ready for use. The toxins, when prepared in this way, are very much stronger than when filtered through a Pasteur, Chamberland, or Kitasato filter, the active principles contained in the germs themselves being preserved. If, as is sometimes the case, the preparation is found to be too strong to use with safety, it can be diluted with glycerine or sterilized water.

The best method of making the bouillon is to soak a pound of chopped lean meat over night in water. In the morning, strain it through a cloth, make up to 1,000 c. c., and boil for one hour. Then filter through a cloth, add peptone and salt, neutralize, and boil again for an hour. The bouillon will then pass through filter-paper perfectly clear, and be ready to put into the flasks. It is not, however, necessary to neutralize the bouillon, as the streptococci will grow even more readily in acid bouillon, and the resulting preparation is, if anything, stronger than when neutralized bouillon is used.

In order to keep up the virulence of the cultures, they are put through rabbits in the following way: The hair of the ear is clipped close with a pair of scissors, and the skin washed with weak carbolic acid, and then sterilized water. A minute quantity of a bouillon culture, forty-eight hours old, is then injected subcutaneously in four or five different places in the ear. Forty-eight hours later, after again washing the ear with carbolic acid and sterilized water, a flat needle sterilized in the flame is inserted under the skin at or near a point of inoculation, and the layer of the skin cut off with a sharp sterilized scalpel. The piece of skin is then rubbed well over the surface of an agar tube with a thick platinum-wire needle. After

twenty-four hours in the incubator, the colonies of streptococci will show as minute white specks, and from them a pure culture can be obtained. If the agar is made with seventy-five per cent. of bouillon and twenty-five per cent. of urine the streptococci will grow more freely than if bouillon alone is used. For the above brief but clear description of that preparation of the toxins that up to the present time has proved most efficacious, I am indebted to Mr. B. H. Buxton. The dose of this preparation varies from one to eight minims; I have had a temperature of 105° F. follow the injection of two minims. I usually begin with the minimum dose, and gradually increase until the desired reaction, *e. g.*, temperature 103° to 104° F., is reached.

In concluding, I wish to briefly summarize my cases thus far treated. Up to May 31st, 1894, I had treated with mixed toxins twenty-five cases of inoperable sarcoma, eight of inoperable carcinoma, and three of sarcoma or carcinoma. In the cases of carcinoma, I had had marked improvement in a number of cases, but no cures. The cases were all very advanced recurrent tumors. In sarcoma, I stated that there were six cases in which I considered there was a reasonable hope of permanent cure. Six months have passed since then, and none of the six cases have shown any recurrence. Of two other cases which were merely mentioned among the tabulated cases as improving, one has gone on to entire disappearance of the very large tumor, and promises to be a cure, and the other, a six-times-recurrent sarcoma of hand, is in perfect health at present, nearly two years since the beginning of the treatment. Since May 31st, 1894, I have treated twenty-four cases of malignant tumors, all inoperable and mostly recurrent, with the mixed toxins. Of these cases, thirteen were sarcoma and eleven carcinoma. In many of the cases of carcinoma, the injections had an undoubted retarding influence, and

in some of the cases the improvement was extraordinary, but in no case did the tumor entirely disappear. In the sarcoma cases, the effect was far more marked, and although in a number of the cases the disease was so far advanced that there could be no possible hope of recovery, still the powerful controlling influence of the toxins was demonstrated, and had the patient's general condition permitted a prolonged use of the injections, the result might have been otherwise.

In three of the thirteen cases, the sarcomas have entirely disappeared, and although no great length of time has elapsed, the results in my older cases make it improbable that relapse will occur. Of my total of thirty-eight cases of inoperable sarcoma, therefore, nine cases promise to be permanently successful. These can be briefly epitomized as follows:

CASE I.—A twice recurrent inoperable sarcoma of the neck, with secondary growth, size of an egg, in tonsil. General condition extremely bad; given by Dr. W. T. Bull three or four months to live. Treatment with erysipelas (living cultures) began May 2d, 1891. No treatment since October, 1891. Result: Tumor of neck practically disappeared. Tonsil tumor greatly decreased in size, and has still further diminished the past year. The patient regained his general health in a few months, and November 12th, 1894, three and one-half years later, remains in good health.

CASE II.—A large recurrent sarcoma of back and groin. Treated at first with living cultures, later with mixed toxins. No recurrence twenty months after cessation of treatment; and nearly three years since beginning. (This case I have had the pleasure of showing you this evening.)

CASE III.—Large inoperable sarcoma of abdominal wall and pelvis. Treatment begun February, 1893. Result: Almost entire disappearance in two months. Patient has been in perfect health ever since, with no tendency to relapse twenty-two months later. (Exhibited at Academy of Medicine, November 15th, 1894.)

CASE IV.—Large inoperable sarcoma of iliac fossa. Almost entire disappearance of tumor. Treatment begun June, 1893. In good health, August, 1894.

CASE V.—Large inoperable sarcoma of the abdominal wall. (Exploratory operation by Dr. Maurice H. Richardson, of Boston. Microscopical examination by Professor Whitney, of Harvard Medical School.) Entire disappearance of tumor under two and one-half months' treatment. In perfect health, December 22d, 1894, without trace of recurrence, fourteen months after beginning of treatment.

CASE VI.—Recurrent sarcoma of leg. (Dr. Bull's case.) Treated at the New York Hospital; later at the New York Cancer Hospital. Entire disappearance of tumor. No recurrence, December 22d, 1894, nearly one year later. (Exhibited at Academy of Medicine, November 15th, 1894.)

CASE VII.—Enormous osteo-chrondro sarcoma of ilium. (Dr. Shrady's case.) Treated at St. Francis and New York Cancer Hospital. Treatment begun March, 1894. Entire disappearance. No relapse, December 22d, 1894. (Since the reading of paper, a portion of thickened tissue at site of old tumor was removed for examination, and no sarcomatous elements were present.) (Exhibited at Surgical Section of Academy, October 12th, 1894.)

CASE VIII.—Sarcoma of hand, six times recurrent. Growing rapidly February, 1893, when treatment was begun. Held in check eighteen months. Degenerated tumor tissue removed July, 1894. Patient has since gained twenty pounds, and is in perfect health, without relapse, December, 1894.

CASE IX.—Sarcoma of scapula and chest wall, involving larger portion of entire left half of thoracic wall. Entire disappearance in three and one-half months' treatment. In perfect health at present (January 13th), two months since treatment was discontinued. (Exhibited at Academy, November 15th, 1894.)

With exception of the first, all of these cases were

NOTE.—By an unfortunate mistake, the photographs illustrating the cases were omitted.

treated with the mixed toxins of erysipelas and bacillus prodigiosus, the streptococcus cultures having been obtained from a fatal case of erysipelas. That the bacillus prodigiosus plays a very important part in the action of the toxins on sarcoma, I have not only repeatedly demonstrated, by using the two singly and combined, but my conclusions have been verified by others. The prodigiosus toxins used alone have a marked effect in causing degeneration and absorption of sarcomatous tissues, but the combined action of the two toxins is greater than when either is used alone. I have also found by experiment that sarcina used with erysipelas has a similar effect.

When we consider that all of the cases reported were hopeless, not only from an operative standpoint, but from any hitherto known method of treatment, that in every case the diagnosis was established according to the most approved principles of medical science, confirmed clinically by the most eminent surgeons, and microscopically by leading pathologists, we are in a position to understand the importance of this subject.

I cannot sufficiently express my indebtedness to Dr. Alexander Lambert and Mr. B. H. Buxton for invaluable bacteriological assistance, and it is largely due to their most careful and skilful work that the preparation of the toxins has been so far perfected as to make accurate dosage possible.

DISCUSSION.

DR. C. MCBURNEY.—This subject of the treatment of malignant tumors with the toxins of erysipelas has interested me extremely, as it must have interested every one who has listened to Dr. Coley's paper. The results reported by him are very gratifying, and open up a field for study such as we have hardly anticipated. There is certainly no denying the importance of the results in the cases he has mentioned, and even if the number that have been benefited or cured had been much less than it is, the interest would still remain. Of course, at this stage in

such an investigation, a great many questions remain to be decided, which can only be answered after further experience. It is difficult to reconcile the great discrepancies between the reports given by Dr. Coley, and those given by a number of other observers. I think, however, that many of the imperfect results obtained by others might be accounted for by the imperfect material used, or by a wrong selection of the cases placed under treatment. Another point that it is difficult to understand, is that these toxins seem to have a specific effect upon one case of malignant tumor, causing not only a cessation of the growth, but even a disappearance of it, while on another case, apparently of the same type, they have no effect whatever.

Personally, my experience with this method of treatment has been very limited, but in the selection of my cases I have had the benefit of Dr. Coley's assistance. In one of these cases, which is still under treatment, the growth, a giant-celled sarcoma, recurred in the stump after amputation at the hip-joint. The injections were commenced about three weeks ago and have been kept up ever since. I cannot say that there is any improvement in this case; the measurements of the tumor are greater to-day than they were three weeks ago, and there is no change in its consistency or color. The growth fills the entire stump, and the case is an inoperable one. This patient has responded to every injection, the fever in every instance being quite severe.

The second case in which the treatment was undertaken was one of multiple sarcoma. There were a large number of tumors, some involving the skin, others lying beneath the skin, in the region of the thorax, back, and both groins, The constitutional effect of the injections in this case was very marked, the temperature sometimes going up to 106° F. In this case the disease has spread, and as there is marked dyspnœa, my belief is that some of the growths have extended to the pleura, and involve important parts within the thorax.

The third case was one of lympho-sarcoma, originating in the thyroid gland and extending to the cervical glands on both sides of the neck. Some months ago I operated with a view to relieving the dyspnœa, but when the operation was about two thirds completed I was compelled to

desist because of the extreme depth of the tumor below the sternum. The patient lived through the summer, and during the past three weeks he has been treated by the injection method. I look upon his condition as quite improved. There is no marked change in the size of the glands, but the dyspnœa has decreased, and I think the sarcomatous tissue has diminished to a certain degree.

I regret very much that the discussion of Dr. Coley's paper has been closed at so early an hour. The subject deserves the most careful investigation, and all surgeons will wish Dr. Coley success in extending and perfecting the treatment of inoperable, malignant tumors.

Anniversary Discourse, November 28th, 1894.

ON THE NEW USE OF SOME OLDER SCIENCES.

A DISCOURSE ON DEGENERATION AND ITS STIGMATA.

By CHARLES L. DANA, M. D.

Gentlemen and Ladies: As I survey the medical activities of the present time, they seem to be working in three distinct streams of fruitful and progressive effort. The first, and just now greatest, takes in all that may be meant by bacteriology or the science of minute organisms. Through this we are learning the secrets of disease and health; through it we shall some day, I hope, reach a germ-free millennium, or at least learn to live unscathed among our little enemies teeming in the air and dust.

Next, one finds that while medical men are abandoning faith in old-time Homeric drugs and drugging, they are working with an intenser energy than ever to perfect the technical details of their art. In every branch exactness of observation, precision in examination, and resourcefulness and ingenuity of treatment are increasing, until the reproach that medicine is not a science is slowly losing its force.

Finally, I see a new and extraordinary energy put forth in the study of the physical characters of man, and the significance of the lines along which each individual is built. We are trying to read in man's general conformation the story of his character, his weakness, his capacity, his power of resistance to disease and temptation—in fine, his moral and intellectual endowments. It is because this movement, though it has reached great proportions in

Europe, has made little impression here that I propose to make one phase of it the subject of my remarks to-night.

It is a doctrine as old as Hippocrates that different people have a special tendency to different diseases, and that they react in different ways to the influences about them; that one will have catarrhs, another gout, another rheumatism, and another consumption; that one is lethargic, another nervous, and another melancholic. It is not of these kinds of investigation that I shall speak; though this new science does in a certain measure what the quasi-sciences of physiognomy and of phrenology, of diathesis and temperament, once claimed to do. It does not seek to accomplish its object, however, by feeling the bumps on the head, or noting the lines on the palm, or studying the handwriting, or by mystic rites performed over a lock of hair. It is a science quite different from so-called phrenology, or physiognomy, or chiromancy—arts which are merely incongruous collections of fatuous guesswork. This coming science has no name that yet specifically indicates its work. It is approached on the one hand by anthropologists, and on the other by students of nervous diseases, of insanity, and of crime, and it receives accessions from the learned savants of the universities and the modest instructors in the gymnasia.

It is a study of the human body in its largest significance, a measurement of the capacity and fruitfulness of the soil. By and by all the harmful microbes of the earth will be subdued, and we shall hear no more of them except as curiosities in germs; but the human soil, on which they used to grow, will remain, with all its ever-increasing complexity, and physicians will then have only the functions of interpreting its values and patching up its injuries or inherited defects.

A sort of dim foreshadowing of this science is found in the history of physiognomy and its allied arts. I use the word and touch upon the subject with some hesitation.

Five hundred years ago Petrus de Abano, of Italy, discoursed upon it and was subsequently burned for heresy, *secundum artem*, in the flames at Padua. Not longer ago than the times of George II. all persons "fayning to have knowledge of Phissiognomie or like Fantastical Ymaginacions" were deemed rogues and vagabonds, and liable to be publicly whipped or sent to the house of correction. The attempt to measure a man by his looks, however, has always been made, and its professional votaries have crystallized their knowledge into many a scientific treatise of admirable length and erudition. In those days there were onychiologists, who read character from the nails; podoscopists, who studied the signs in the feet; metaposcopists, who read the face, and many other subdivisions of the general art of fooling people by examining certain fractions of their body. The ancients, however, did not build their science up securely; they could not resist the desire to mingle prophecy with their imperfect divination, and as time passed and knowledge grew, both prophecy and the various arts of physiognomy declined.

A little over a hundred years ago a devout clergyman of Switzerland, Louis Lavater, gave the last impulse to this line of work in a most learned, pious, and industriously written treatise on physiognomy, which he dedicated to the promotion of religion and right thinking. Lavater called attention to the striking way in which physiognomy sometimes indicates the character and intelligence of the individual. Perhaps he overdid the thing a little. His pictures of bad men were so frightful that the reader's vanity was almost compelled to feel that he must be good, while his virtuous faces were so insipid as to excite self-respecting men to a distinct desire for vice.

The soul and intelligence, said Lavater, show themselves in the head and brow, and their center is the eye; the moral life is indicated in the face, and its expression centered in the nose and cheeks; the animal and physical

life are centered in the stomach. They showed their predominance also in the mouth and chin.

One day Lavater made the mistake of thinking that an executed assassin was an eminent philosopher and preacher, Herder. Worse mistakes have been made, but still such things impaired the dignity and security of his already decadent system. Lavater's teachings were too simple in theory for a science and too complicated in practice for an art. His illustrations remain, and his head of Satan in particular represents a character which no one can see without desiring to resemble it. His physiognomy died in the arms of a more successful form of philosophy.

It was only a few years later that an Austrian physician, Dr. Gall, began his lectures on what is still known as phrenology. The story of how he noticed the bulging eyes of his school-fellows who surpassed him in their memory, and of how he found big bumps over the ears of murderers and over the temples of thieves, is probably familiar to many. It was at least part of the learning of my school-days. Dr. Gall was an industrious enthusiast, and there is little in the phrenology of to-day which is not due to his labors, though he had able helpers and successors in Spurzheim and Combe. Practical phrenology now is really but a shrewd empiricism based on physiognomy, and not upon the classical bumps at all.

Taking a standard phrenological bust as sold by the professors of the art, I have drawn over the various faculties, as indicated by Gall, lines by which I could map out the functions of the different parts of the brain, so far as they have been absolutely determined by modern investigations. These show some curious juxtapositions. Thus the bumps of murder or destructiveness, and secretiveness which lie just above the ears, correspond with the center for hearing; the bump of thieving, which is a little in front of the ear, lies over the center for the movements of the mouth; while self-esteem and reverence

correspond with the more measured activities of the legs. I find also that parental love is identical on the bust with the sense of sight, and hope with the motions of the shoulders and arms. Perhaps some philosopher of the future will reconcile these differences and construct upon them an esoteric metaphysics; but at present it seems to me that physiology has quite extinguished the science of bumps. While the actual contributions of Dr. Gall to science were small, his work called attention to the importance of studying the conformation of the head and the function of the brain, and gave an impetus to more serious and fruitful studies.

It is since Dr. Gall's time that the word "degenerate" has become one of common usage in French literature, running through works on heredity, social economy, art, morality, crime, insanity, and disease in all its forms. We find French literature full of articles upon degeneracy and civilization, degeneracy and crime, degeneracy and disease. There is in France a whole school of the Decadents, as they more euphemistically are called. The term has been adopted by the Germans, and runs through the work of Lombroso and his followers in Italy. In England and this country its use and significance seem to me to be much less appreciated. Yet since the first applications of the new science have been toward helping us recognize those who belong to the class known as degenerate and unstable, I shall devote a little time to explaining what is meant by the word.

The term has an unpleasant sound, and nobody likes to hear dry expositions of depressing phenomena. The New Yorker of to-day will hardly tolerate a tragedy; he wants his drama farcical, his art sensuous, and his novels with a pleasant hymeneal ending. I cannot believe it possible for me to make a discourse on degeneracy at once lucid and attractive. But if one will pass the tip of the tongue over the roof of the mouth, five or six out of

every one hundred adults will feel a curious ridge along the middle of the hard palate. This is called the *torus palatinus*, and whoever has it possesses one very interesting mark of degeneracy. The finding of this will, I am sure, give a certain percentage of people a personal interest in the subject, and will make them listen with some quietude in order to learn if it has any painful significance.

I do not need at this day to refute that shibboleth of the Reign of Terror that all men are born equal. We all know very well that babies start in life with very unlike endowments. For the purposes of my explanations, however, I shall only say that some are born like and some are born unlike their parents. Those who have more or less fully the mental and physical characters of their family are considered technically normal, while those unlike it are degenerate. The degenerate are those who differ from the average standard as set up by the family from which they spring; they are variations from the healthfulness of mediocrity; they are the peculiar people. Here, for example, is an illustration familiar to all. An honest, healthy, and respectable couple have a number of children; most of them take after their parents more or less closely and grow up honest, healthy, sturdy, perhaps mediocre or somewhat more than mediocre men and women. But one of the children is different: he is wilful, perverse, impulsive, deceitful; perhaps, however, talented in music or art, in business or literature. He is looked up to as the genius or the doubtful character of the family, and all the friends wonder where he got his endowments. He is one of the degenerate. If his talent overrides his moral imperfections, he makes, perhaps, a success of life, and he is called, technically, simply an unstable, or, popularly, an erratic man. If his talents and bad tendencies are pretty evenly balanced, he is called a degenerate of the superior type, while if he discloses decided mental

weakness or develops an epilepsy, or hysteria, or some curious form of hypochrondriasis, he is called an inferior degenerate.

From this last class are fed also the insane and many criminals. The weak-minded, imbecile, and the idiot stand at the lowest level of the degenerate class. The common characteristic of all is that they are variations from the family average, they have mental faculties and physical powers not evenly balanced, and if they marry, they may accentuate the degeneracy, while eventually their family becomes sterile and runs out. This tendency to sterility is the final and uniform criterion. The histories of prominent families of Europe, aristocratic and royal, are often cited as illustrations of this phenomenon. But it is not confined to them, and we see it in the country as often as in the cities.

There is, however, a brighter side to this picture. The human race possesses a conservative force which constantly tends to bring individuals back to the average and healthy type. Giants do not breed giants, or dwarfs, dwarfs; neither do idiots breed idiots, despite the reiterations of controversial journalism, nor do criminals breed criminals. These classes have no children, or their children are better than the parents. Men and women of genius, and even those of extraordinary talent, follow the same rule. Their children tend to drop to the level. There is a centripetal force that brings the generations back to the type of the average man. No amount of acquired aptitude or special skill is transmitted to the children, though some of that with which one is naturally endowed may be passed on. The degenerate class in all its grades is a very large one, embracing five to ten per cent. of adults; few successful men can be said to be free from the taint. And since perfect stability of the organism is only found in the average, or some would say, commonplace man, it may be said that degeneracy is a con-

dition not altogether undesirable. Such a view is quite justifiable. I am flying in the face of the old adage, that the best possession in life is *mens sana in corpore sano;* but I do not think that perfect health and perfect sanity are things that any individual should desire. It is good for society as a whole, but not so for the individual, or, I might parenthetically and professionally add, for the doctor.

I have often noticed that families with a streak of insanity or inebriety in their membership number the brilliant and successful men. Only the insanity, like the boil, should affect the other man. No man of genius had ever perfect physical proportions or perfectly stable organizations. On the other hand, the portraits of beautiful women are not the portraits of women who have had great intellectual attainments, and the beauty of women has only affected the world's history indirectly through lower channels. Perfect beauty means mediocrity, and man's desire for the one is the force which brings back the degenerate to the other.[1] There is a scientific justification for the fun which one of our daily papers so often pokes at pulchritude. I do not mean that one must be ugly in order to be brilliant, but only that in women and men of talent or wickedness there is some deviation from the standard type which the modern sanitarian would call the normal.

But here I must make some important modifications in my text. I have spoken as though there were but two classes of people: (1) The normal, which includes the great majority; (2) the degenerate. All writers have so far chosen to make only these distinctions and have estab-

[1] The well-known artist, Mr. A. B. Frost, is quoted by Robert Bridges (The Bookbuyer) as saying, referring to artists' models: "I might mention that there is a singular peculiarity about the women. All the handsome ones are stupid and can't put an idea into a pose, and all the bright ones who can and will pose and help you work, are decidedly plain both as to face and figure."

lished the signs by which to divide all humanity into the sheep and the goats.

But in this, as it seems to me, one important element is left out, or not sufficiently considered. While most people may be what the modern esthete or green carnation critics call "middle class," the remnant are not all below the average, as their name "degenerate" would imply. Assuming at least that the human race is slowly progressing, it must be that the average man and woman is also becoming of a slightly higher type; and this in turn must be because many of the variations from the healthy average are distinctly above it. Some of the people who have the peculiarities of instability and tendency to die out, do nevertheless give birth to a more vigorous stock than they themselves possess. In other words, unless the world is growing worse there must be a class of greater than average vigor, who may be called the progressives or "regenerates." There are, however, as yet no anatomical marks known which distinguish these as a type.

I hope I have made measurably clear the idea of degeneracy as it has been developed and taught by latter-day writers. It forms a class of which the members are as far apart morally and intellectually as the possibilities of human nature allow. Yet it has common characters in the reproductive weakness, in the defects of physique, and in the instability of their mental faculties. They are all wanderers from the normal lines. Furthermore,—and this is the point to which I have all along been, very slowly, I fear, tending,—they have about them certain marks or *stigmata* which indicate their divergence, and are badges of their servitude or of their freedom. The study of these marks forms the application of modernized physiognomy and anthropometry to the determination of the degenerate classes. This is the new use of the older sciences.

In France, Moreau, Magnan, Féré, and others have followed up the work of Morel, who may be called the father of the study of degeneracy. But undoubtedly the greatest impulse has been given to these studies by Professor Lombroso and his pupils. This eminent scientist believes that criminals are unlike their own race, but resemble each other and are a family among themselves, with a family likeness and common bodily peculiarities. This conclusion is one to which I cannot at all agree, and which I feel sure is not yet subscribed to by men of science. The exact position in which Lombroso's work stands, I shall refer to later. Meanwhile, one cannot overestimate the value of the impulse which he has given to investigations of the human body. If there is ever to be a complete and exact science by which we can in a measure infer character from figure, it will be very largely due to the work of the great Italian criminologist.

I propose now, however, to go into some of the details of the curious discoveries that have been made by investigators, not in Italy alone, but in all parts of the world. I do it with the hope that my descriptions will not lead to any unpleasant habits of self-examination or to surprises that act disagreeably upon one's vanity. I shall take up the points somewhat at random, for I have not time to present the subject in any systematic and complete manner.

Of all the various systems of which the body is composed, the bones have been thought to be allied most completely with the development of the nervous system. Sir William Gull once suggested that the Socratic advice, γνῶθι σεαυτόν, should be interpreted in these days "examine your urine;" but the modern physiognomist would rather say, "examine your bones."

A perfect bony system is a rather good index of vitality at least, and its variations and anomalies make up a large part of the stigmata of degeneracy. The presence of

excessively long arms, for example, is a phenomenon greedily seized upon by the Italian school as indicating a return to man's ape-like state, before he fell from grace and his arborial gambols in primeval forests.[1] But long legs and a small chest are also found to be evidence of degeneracy; and whatever may be said of the marks written on the bones, none of them prove that their anomalies are signs of a return to savagery. Degeneracy is not a form of atavism.

Naturally the conformation of the head and face has been studied with much earnestness and much practical success. The skull of man is never quite symmetrical, but it tends to become so; on the other hand, the brain tends to become asymmetrical, *i. e.*, the left side to be a little larger than the right. When things go a little wrong in the human make-up, the skull shows it quickly by getting out of shape. The size does not count very much, for we know that men of genius, like Gambetta, have had skulls that would barely be large enough for a boy. But odd shapes do signify. In olden times, the physiognomists thought that men of evil character had a sugar-loaf (oxycephalic) skull, while Gall found his murderer's bump over the ears. And modern criminologists lay much stress on the size of the jaws.

There are at least thirty ways in which the head varies from the normal type. Many of these are most trivial, and it is only on the principle that a dozen small things make an object of respectable size that one is justified in giving attention to the facts that are collected. Ordinarily we look in a man's face to see whether it is in general attractive, intelligent, interesting, beautiful, or the reverse. But the anthropologist looks more closely, and hunts for his stigmata. One third of all thoroughly

[1] When the arms hang straight down by the side, the tip of the middle finger is 14 ctm., or about six inches, from the knee-cap. In negroes it is only five to eight ctm., or two to three inches (Krause).

bad men, for example, according to Dr. Penta, have a plagiocephalic or twisted-shaped skull.[1] A still larger proportion,[2] forty-five per cent., have projecting jaws, or prognathism, wide jaws (twenty-nine per cent.), and wide orbits (thirty per cent.).

The nose in the criminal is often rectangular, short, large, and upturned; the face shows early wrinkles and a peculiar pallor. The criminal jaw weighs 94 gm. (3 ounces), the normal jaw but 80 gm. (2½ ounces) (Manouvries). The cheek-bones are prominent, the orbits are large, the frontal sinuses and brows protrude, and the teeth are defective and badly placed. The shape of the palate stands in close relation to that of the skull, and the neat and workmanlike way in which Nature joins the parts in the middle shows that she has done well elsewhere. When the palate has a ridge along its centre, *torus palatinus*,[3] or is high and narrow, or uneven and badly shaped, we count it among the signs of degeneracy, though it weighs lightly in the scale. Criminals have three or more of these various anomalies.

The size of the head, as already stated, counts perhaps less than anything else, unless the deformity be greatly exaggerated. The good and the bad, the sane and the insane, have heads of about the same size, and even prehistoric skulls do not show much inferiority. Civilization began and completed its work with almost precisely the same amount of brain tissue.

The anomalies above described have a rather forbidding and perhaps alarming sound, and it is pleasant to know that individually they are not very significant.

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[1] Arch. de Psych., vol. xv, p. 327.

[2] *Ibid.*

[3] Among 71 patients in the general wards at Bellevue Hospital, I found a distinct *torus* in four, and in two of these cases the patients had nervous disease. In 57 patients examined in the alcoholic pavilion, I found the *torus* in 11 cases, or twenty per cent. The *torus* is more marked in inebriates than in the insane, though palates otherwise deformed are more frequent in the latter class.

A homely face can be quite free from the stigmata of sin, while one of a fascinating and even beautiful type may have much that suggests degeneracy.

There are, as artists and anatomists know, certain definite proportions between the circumferences of the neck, shoulders, chest, waist, and hips. These proportions differ for infants, men, and women.[1] Among the unstable there are often found misfits in these proportions, so that a man for example, has approximately a child's or woman's proportions. This condition is known as feminism or infantilism, and many striking illustrations of it have been shown by Dr. Charles Féré, of Paris. I have, I am sure, seen it in some classical statues, and I believe it will be found that the Greeks had a certain unholy admiration for some of these beautiful but perverted types of the human figure.

We every-day people have rather agreed to ignore the ears, yet it is remarkable to what uses they lend themselves at the hand of science. Laycock thought that the circulation in the ear was an accurate indication of that in the brain, and that its development coincided with that of the brain and skull. The ear, to him, was to the brain as accurate an indicator as the tongue is to the stomach. Mr. Ellis speaks of an observant schoolmistress who could tell when her pupils were tired by the state of the circulation in their ears. Professor Grandino, of Turin, however, may be said to have done the ear the most illustrious service. He has examined these organs in twenty-five thousand healthy men and women, besides

[1] The average circumferences in men and women are given by Krause as follows, in centimetres:

	Men.	Women.
Circumference of neck	34	32
" at level of iliac region	70	73
" level of hip bone joints	81	84
" breadth between trochanters	34	35
" shoulder breadth	39.1	35.2
" hip breadth	30.5	31.4

eight hundred insane and four hundred and sixty-seven criminals. I have before me the typical ear as it should be, or at least as the professor depicts it from the results of his laborious studies. I must confess that after having mastered its admirable symmetries I have gained a new experience in æsthetics. For now it gives me a distinct delight when I come across a perfect aural organ, such as one experiences in seeing a beautiful picture or listening to the harmonies of an up-to-date sonata.

It is surprising how many shapes the ear may take, when one comes to look at it seriously in the light of a newly growing science which is thirsty for morphological data. There are at least seven distinct ways in which the ear can be wrong; each corresponding perhaps to a deadly sin, for each is more prominent in the sinner than in the saint. I am not going to enumerate these points, for I cannot do it except in the most technical language, and besides, I do not like to give people who are kind enough to listen to my discourse any opportunity to form a poor opinion of themselves. The long lobes and a peculiar wrinkle in the cavity of the ear are the things which have most significance.

One of the most curious things discovered by the patience of the neurologist has been the prehensile foot *(pied préhensile)*. Drs. Ottolenghi and Carrara examined the toes of one hundred and sixty-two normal people, thirty-one epileptics, fifty-six idiots, and three hundred and fourteen criminals.[1] Among normal people the distance between the first and second toe at the base rarely exceeds 3 mm. (1-8 inch) in men, while in the abnormal classes such excess is three times as frequent. The idiot classes seem to be especially marked with this stigma, so that it would be inferred that the activity of the intelligence is indicated in a measure by the distance between the toes. This would seem absurd, were it not

[1] Giorn. della R. Acad. di Med di Torino, 1892, xl, 423.

that the peculiarity suggests a return to the prehensile toe of the ape, and is therefore classed as a mark of atavism.

I said some time ago that chiromancy had not been taken up seriously by scientists. The lines and furrows which have been studied as a parlor game, and about which prophetic and pseudo-scientific works have been written, is mainly but guesswork. Yet the lines so delicately arranged on the finger-tips are beginning to assume some significance. Mr. Galton, in his work on finger-marks, established the fact that they never change in the same individual, and that they may be to a certain extent classified. His investigations were confined to normal people. Alix did the same for the finger-tips of apes, and now Forgeot, of Lyons, says that in young thieves of Boulogne are traces of the same arrangements seen in apes, and Féré finds them among sixteen per cent. of epileptics.[1]

Much has been written about the hand; artists have depicted its perfections, physiognomists have interpreted its shape and expression, chirologists and the gypsy queens have tried to prophesy from its markings. But a study of its peculiarities by the inductive method takes one into entirely new fields, and shows that to interpret the significance of the hand one must start on quite new lines. There is something, perhaps, in the fact that the artistic hand has somewhat tapering fingers, but beyond this one deals with uncertainties. And I recollect an idiot youth whose extremities had the contour of a genius or a Trilby.

If one examines the hands of a large number of persons with a nervous endowment, he will find curious defects in the length and relative proportions of the fingers. The length of the finger is determined by that of the middle one. If the index and medius are closed upon

[1] Les Empreintes digitales, Lyon, 1891.

the palm, the ring and little finger being left extended, the middle finger will reach close to the place where the so-called life-line runs down, between the ball of the thumb and that of the little finger. It will touch the palm just below the highest part of the ball of the thumb.[2] The middle finger is taken as the standard of length by which to gauge that of the others. In a normal hand the fore-finger reaches just to the root of the nail of the middle finger, the ring-finger is longer and should reach nearly to the middle of the nail of the medius, while the little finger should reach to the last joint of the third finger. Now in inebriates, epileptics, neurotics, and the degenerate generally, these proportions are often not observed. The most common defect is shortness, especially of the third and little fingers, though sometimes a disproportionate length occurs. Sometimes these fingers are unnaturally slender or the little finger is slightly bent. The most common abnormality of the thumb is excessive shortness, with a defective mobility. These peculiarities well accentuated form what we may call the "decadent hand"—the hand that writes our sensuous novels, the Hauptmann drama, paints symbolic pictures, and exploits pure æstheticism. Such hands may be well formed to the ordinary eye, and may be attached to slender and graceful limbs. But this kind of beautiful hand and arm is found quite as often among the children of alcoholics and among those highly cultivated families which have become degenerated by vices and vicious crossing.

The hair adds a little to our knowledge of the individual, but not very much. Light hair and blue eyes seem to be dangerous endowments in the Italian race, but this is not the case with the Anglo-Saxon. It may be a comfort to know that hair grows plentifully on the heads of criminals, and that prematurely bald men are rarely bad, through any hereditary defect at least. The degenerate

[1] Féré: La Famille neuropathique, p. 281.

has often a thick head of hair and a thin beard, while in modern society the normal man seems to be equipped in just the opposite way. That twist of the hair known as the crown of the head, should be in the middle line, or very close to it, and no one is entitled to more than one crown. When it is found shifted off well to one side, or is doubled, the twist becomes a stigma. Art and fashion permit one to tip the hat slightly to the side, but nature orders that we wear our crown in no jaunty or frolicsome mode. It is often thought that an abundance of hair signified a vigorous vitality, but this is not strictly the case. The excessive development of hair on the body, particularly in women, is regarded as one of the minor stigmata of degeneration.

My remarks would be both incomplete and misunderstood if I left it to be inferred that degeneracy, in its various forms, is determined by bodily marks alone. There are two other classes of stigmata, the physiological and the mental. Under the former, we include peculiar defects in the function and practical working of the body; under the other, we include certain mental characteristics. The degenerate are apt to have an unstable circulation and a deficient vitality that responds abnormally to stimulants of various kinds. Mentally, a peculiar characteristic is a tendency to morbid fears, introspection, idiosyncracies in taste, and impulsive acts. These and many other of the stigmata occur in the normal, however, and I do not propose to enter a branch of the subject which is much less definitely worked out and less susceptible of demonstration.

I have now described to you the gradual development of a science of human measurement out of the cruder sciences of physiognomy, temperament, and phrenology.

The metaposcopist of the Greek school viewed the body and detected in it signs of some dominant quality. The

man was quick and irascible like the dog, or slow and sensuous like the pig, brave and forceful like the lion, every one resembling in feature or character some lower animal. The mediæval artist added to this the science of divination and prophecy. In the times of Lavater the physiognomist tried to interpret from the attitude, the complexion, and features, the various complex elements of the individual's disposition.

The phrenologist of the Gall school went still further, and described all the intellectual and moral faculties, mapping out for the man the total sum of his capacities, his aptitudes, and his prospects in life.

The modern student of the human frame is much more modest. He asserts only that he can by the presence of certain stigmata say whether the individual is of stable and normal organization, or if he belongs to that class which I have described as the degenerate. Nor does he pretend to do this in all, but only in those whose defects are well defined and reasonably numerous. Perhaps every one has some stigma of degeneration; but at least five out of a hundred adults have many and striking ones. The degree of their degeneracy may be in a measure decided, but cannot be determined by anatomical marks alone. And it is on this point that I must differ from the Italian criminologists, who claim that there is a criminal, an insane, a neuropathic type.

In conclusion, let me say that the final work of modernized physiognomy will not be to fix upon human beings any stigma which marks them as necessarily useless, defective, or dangerous. We have not discovered a type of criminal man, or of the insane man, or epileptic or neurotic man. All these have common marks which show simply that they belong to the same somewhat handicapped family. Their presence in an individual will show that he must be especially careful in educating and using his natural power. The discovery of degeneracy throws

an additional responsibility upon him, for there are few so bad but that with a proper environment they can get along successfully in life.

There may be some born criminals, but they are very few; most are simply persons of degenerate type who fail to husband properly the endowments they possess. We do not excuse the cripple who attempts to become a sprinter, nor should we excuse the morally defective who indulges in debasing habits and low temptations.

All modern studies in the lines I am exploiting seem to show that man must be more than ever careful of his education, his training, and his surroundings, and of using all possible moral and spiritual agencies to strengthen his defects and make more stable his powers. The future of the degenerate depends enormously upon these factors, and his responsibility lies in his following that line of life which is right for him. By this means one can render many defects harmless, and make the unstable almost as firm in judgment and healthful in body as the happy possessors of perfectly balanced organizations.

If I have succeeded in showing that this is to be the end and teaching of modernized physiognomy, and shall have aroused your interest in its progress, my object will be accomplished to-night.

Stated Meeting, December 6th, 1894.

THE TREATMENT OF DIPHTHERIA, INCLUDING SERUM THERAPY.

By H. W. BERG, M. D.

The term diphtheria up to a decade ago had so vague a significance that even the best authorities were not agreed as to what clinical or pathological conditions were embraced under it. While some gave the name to all so-called croupous inflammations of mucous membranes with the production of pseudo-membrane, others limited the term to an acute infectious disease of a constitutional character with local, generally throat, lesions, the local lesion being generally characterized by the production of false membrane. The researches of Klebs,[1] Loeffler,[2] Prudden,[3] Roux,[4] and Yersin, and a host of others, have taught us much more about the nature of diphtheria than perhaps is known about any other acute infectious disease. They have taught us that while clinically true diphtheria resembles other similar conditions embraced under the term of pseudo-diphtheria, the two groups of affections are radically different in their etiology viewed from a bacteriological standpoint.

Although the purpose of this paper is not to study the bacteriology of diphtheria, but rather its treatment, yet, in view of the light thrown upon the treatment of diphtheria by our knowledge of the bacteria which cause it, an apology for prefixing a *résumé* of the main points in

[1] Klebs: Verhand. des Congres f. In. Medecin. 1883, ii, 139.
[2] Loeffler: Mittheil. aus. d. Kais. Gesundheitsamte, 1884, ii, 421.
[3] Prudden: Amer. Jour. Med. Sciences, 1889, xcvii, 329.
[4] Roux and Yersin: Annal. de l'Inst. Pasteur, 1888, ii, 629.

the bacteriology of diphtheria to a paper on the treatment of the disease is hardly necessary.

In 1883 Klebs first described a characteristic bacillus, which he had found in the false membrane in the throats of patients dying from epidemic diphtheria. In 1884 Loeffler carried these investigations much further, confirmed Klebs's discovery of a characteristic bacillus present in the pseudo-membrane of fatal epidemic diphtheria, and succeeding in isolating the Klebs bacilli from the bacteria accompanying it, obtained the bacillus in pure cultures. He was able to inoculate the abraded mucous membranes of the trachea of susceptible animals with pure cultures of this bacillus, and thus obtain the local manifestations of diphtheria and subsequent death; while the hypodermatic injection in guinea-pigs of a quantity of bouillon culture of the bacillus resulted in death with the customary lesions of diphtheria.

Park describes these bacilli as varying in diameter from 0.3 to 0.8μ., and in length from 1.5 to 6μ. They occur singly and in pairs, and very infrequently in chains of three or four. The rods are straight or slightly curved, and usually are not uniformly cylindrical throughout their entire length, but are swollen at the ends, or pointed at the ends and swollen in the middle portion. Even from the same culture different bacilli differ greatly in their size and shape. The two bacilli of a pair may lie with their long diameter in the same axis, or at an obtuse or an acute angle. The bacilli possess no spores, but have in them highly refractile bodies. They stain readily with the ordinary aniline dyes, and retain their color after staining by Gram's method. With an alkaline solution of methyl blue, the bacilli, from blood-serum cultures, especially, and from other media less constantly, stain in an irregular and extremely characteristic way. The bacilli do not stain uniformly. Loeffler did find some cases of so-called diphtheria in which these bacilli

were not present, and in the light of our present knowledge it would have been remarkable if every case examined had shown the characteristic bacilli. For cases of pseudo-diphtheria are not distinguishable clinically from true diphtheria, so that presumably some of the cases examined by Loeffler were cases of pseudo-diphtheria.

In this way, too, we can account for so excellent an observer as Dr. Prudden being unable to confirm, in a series of fatal cases of diphtheria taken from New York hospitals, the results obtained by Loeffler. Most of Prudden's cases were diphtheria complicating scarlatina, which in most cases is only so-called streptococcus diphtheria. But in his second contribution to the study of this subject this same authority has confirmed the findings of Loeffler. It remained for our own city, however, to test the question of this constant occurrence of Loeffler bacilli in true diphtheria upon a scale hitherto unequaled.

In 1893 the New York City Board of Health instituted a series of accurate bacteriological examinations of all cases of suspected diphtheria reported to the board as occurring in the city of New York. This work was performed under the direction and supervision of Dr. Herman M. Biggs, by Dr. William H. Park and Mr. Alfred L. Beebe, and from the report of these latter gentlemen to Dr. Biggs[1] much interesting and valuable information concerning diphtheria that occurred from May 4, 1893, to May 4, 1894, can be gleaned. There were 5,611 cases of diphtheria reported and investigated bacteriologically, in which Loeffler bacilli were present in 3,255 of these, in 1,540 no Lffloeer bacilli were found. Eight hundred and sixteen cases showed no Loeffler bacilli, but for many reasons, probably often through faults of technique in inoculating culture-tubes, and for other reasons, the results of the investigations were unreliable. Excluding these cases, then, about sixty-eight per cent. of the cases relia-

[1] Med. Rec., 1894, xlvi, 385.

bly observed were cases of true diphtheria, as shown by the presence of the characteristic bacillus, overwhelming evidence, even if no more were learned from these investigations, that the Loeffler bacillus is the active agent in the production of true diphtheria. The belief, then, that the Klebs-Loeffler bacillus is the specific infectious agent in diphtheria rests upon the following data:

1. It has been seen by every competent observer, and in enormous numbers of cases, at the site of the pseudo-membrane, both by cover-glass examination of the exudate, and by examination of cultures made by inoculating culture-tubes with the exudate.

2. This bacillus is found only in true diphtheria, or in diseases complicated by true diphtheria; although Park concludes, from a series of investigations, "that virulent diphtheria bacilli are to be found in the throats of a small proportion of healthy persons throughout the city, and that they have been derived either directly from diphtheria cases or from those who have been in contact with them."

3. Pure cultures of this bacillus induce characteristic diphtheritic inflammations when inoculated into certain of the lower animals, followed by local paralysis and other clinical manifestations of true diphtheria. Thus when such inoculations are made into the trachea of rabbits they cause membranous croup and tracheal pseudo-membrane, with œdema and swelling of the tissues and swelling of the glands of the neck.

Loeffler, Welch, and Abbott,[1] and others have found that when susceptible animals are inoculated with pure cultures of the diphtheria bacillus, and diphtheria results, the bacilli are only found at the site of the inoculation. Cultures made from the blood of affected animals never showed bacilli. Martin, Behring, and Frosch, however,

[1] Bulletin of the Johns Hopkins Hospital, February and March, 1892.

have found, although rarely, bacilli in the blood of animals thus affected.

It would thus appear that in true diphtheria we have an acute infectious disease with constitutional and local symptoms; the local inflammation is very generally characterized by the production of pseudo-membrane, in and about which Klebs-Loeffler bacilli are found. Sometimes, however, these bacilli are found in throats that show no sign of false membrane, but have simply a congestion and swelling of the mucous membrane, with more or less constitutional manifestations. Most observers agree, Park among others, in considering that the presence of the Loeffler bacilli, with inflammatory swelling of the mucous membrane, is sufficient to stamp these cases as true diphtheria, and that they should be so treated.

The well-known constitutional symptoms frequently so great as to cause death from the severity of the septic manifestations, or in other cases well-known sequelæ due to a profound toxema of the nervous system, and to inflammatory changes in distal organs, logically render it absolutely necessary to suppose that since the pathogenic bacillus is only found locally at the site of the lesion in the mucous membrane, the constitutional symptoms are the result of some toxic agent produced by or under the influence of the bacillus, and spread throughout the system by the lymphatics and blood.

Experimentally, a bacillus which destroys the life of a susceptible animal when injected beneath the skin or mucous membrane, and which gives rise to severe constitutional symptoms at a distance from, although itself only present at, the point of injection, must owe its power to the production by its agency of a toxic material which is carried by the lymphatics and circulation to distant parts of the body.

In 1888 Roux and Yersin made observations tending to show the pathogenic power of cultures of Loeffler

bacilli filtered through porous porcelain. In a later contribution to the study of this subject they showed that the reaction of recent cultures which had been filtered was acid, while old cultures similarly filtered had an alkaline reaction; the recent-filtered acid cultures were less harmful, comparatively, than old alkaline reacting cultures, when injected into animals. As to the chemical nature of the poison present in these filtered cultures of the true diphtheria bacillus, Roux and Yersin believe that it is a diastase. The toxic power of this poison is very much reduced by exposure to a temperature lower than 58° C. for two hours, while subjection to a temperature of 100° C. for twenty minutes completely destroys it. This toxin is insoluble in alcohol, and can be precipitated by alcohol from old cultures. It appears to be an albuminoid substance, but its exact composition is unknown. We have, then, clinically, in true diphtheria to treat a communicable infectious disease, which is characterized by a local lesion at the site of the Loeffler bacilli, and constitutional symptoms due to the pathogenic activity of a tox-albumin brought into existence by the influence of the bacilli. It will, therefore, be proper to discuss the treatment of the affection under the following heads:

1. The prophylactic and preventive treatment, including the discussion of the conditions under which the disease is communicated from one to another.

2. The treatment of the local lesion.

3. The treatment of the constitutional effects of the pathogenic action of the toxin secreted by the Loeffler bacilli.[1]

Prophylactic Treatment.—In the prophylactic treatment of true diphtheria we have the advantage of knowing the pathogenic factor we have to combat, namely, the Loeffler bacillus. This bacterium, as I have already stated, is found for all practical purposes only at the site

[1] Annales de l'Institut Pasteur, 1888, ii, 629.

of the local lesion; in the vast majority of cases this is in the throat and upper air-passages. We find it most plentifully distributed in and about the pseudo-membrane. Shreds of this membrane are the fruitful nidus of the bacterium. Competing, however, with the false membrane in respect to a plentiful deposit, are all the discharges from the site of the pseudo-membrane, and even on the surface of mucous membranes in the neighborhood of the disease, not the site of pseudo-membrane, the Loeffler bacilli can be readily obtained by culture.

In 80 per cent. of cases occurring in New York city, during the past year, of so-called croupous laryngitis, Loeffler bacilli have been found by simply swabbing the posterior wall of the pharynx. Thus the discharges from the nose, mouth, throat, and larynx of patients suffering from true diphtheria are highly infectious, and through them the disease is communicable. Not only is this the case, however, but the throats of those coming in contact with patients suffering from diphtheria have been found to contain Loeffler bacilli while in health. In some of these, diphtheria developed later on; in others, diphtheria did not develop in the affected individual, but contaminated other persons with true diphtheria.

Park estimates that "virulent diphtheria bacilli were present in about one per cent. of the healthy throats in New York city at the time of these examinations (May, 1893, to May, 1894), and most of the persons in whose throats they existed had been in direct contact with cases of diphtheria." "The members of a household in which a case of diphtheria exists should be regarded as sources of danger, unless cultures from their throats show the absence of virulent diphtheria bacilli."

As to the length of life of these bacilli after they have been discharged from the body, it is found that their virulence persists for a long time. Loeffler and Park found that the true diphtheria bacillus maintained its life upon

blood serum for seven months. Roux and Yersin found that they maintain their life in dried shreds of membrane for twenty weeks. Thus the dried sputum upon clothing, toys, and the like, might infect five months after the clothing had been worn by a diphtheritic patient. In my own practice two severe cases of diphtheria had been treated late in spring, and on recovery the patients were sent away to the country. The sick-room was not again occupied until November; it had been, in the meantime, thoroughly fumigated. Two weeks after the return of the children to this room, a severe attack of membranous croup occurred in the youngest child, of which the little patient died.

A proper bacteriological examination is the most important factor in the prevention of diphtheria. The method of making a bacteriological examination of the discharges from a suspected case is as follows: A sterilized swab of cotton and tube containing sterile solidified blood serum are the necessaries for making a culture. The swab is rubbed gently but firmly against the suspected mucous membrane if no exudate be visible, then immediately on removal inserted into the serum tube, and the wet portion of the swab rubbed gently and yet thoroughly upon the surface of the serum. Thus the culture tube is inoculated. This tube is then placed in an incubator, and kept at 37° C. for twelve hours. It is then ready for examination. A slide is then made for the microscope. A trace of water is placed upon a clean cover-glass; a sterilized platinum needle takes up a little of the surface of the culture medium where most of the colonies have grown; this needle point is rubbed upon the drop of water on the cover-glass, until the bacteria are supposed to be evenly distributed upon it. The cover-glass is allowed to dry in the air. It is passed rapidly three times through the Bunsen flame, covered with a drop of Loeffler's solution of alkaline methyl blue, and allowed to dry for ten minutes.

It is washed in clean water, dried, and mounted in balsam. It may be examined with $\frac{1}{12}$ immersion lens.

We find that the patient himself is the most fruitful source of contagion. Those who come in contact with the patient, including the nurses, relatives, and friends, and even the physician, are further agents for spreading the bacteria; and, thirdly, that the furniture, utensils, clothes, and even the walls themselves, become contaminated with shreds of membrane and discharges containing Loeffler bacilli, and that all of these may become sources of infection to those in health.

As to the question, how long the patient is a source of contagion; that depends upon the length of time that the bacilli can be obtained by culture from diseased mucous membranes. The bacilli persist in most cases after the pseudo-membrane has disappeared. The length of time that they persist varies partly with the severity of the case, partly with local treatment adopted. Of 605 cases that recovered, Park and Beebe found that in 304 the diphtheria bacilli disappeared within three days after the disappearance of the exudate; in 176 cases the bacilli lasted for seven days; in 64 cases, for twelve days; in 36 cases, for fifteen days; in 12 cases, twenty-one days; in 4 cases, twenty-eight days; in 4 cases, thirty-five days; and in 2 cases, sixty-three days after the disappearance of the exudate. In all of these convalescent cases the Loeffler bacilli, although growing gradually less numerous and becoming more admixed with cocci, are still virulent generally to the end. It becomes highly important as an element in prophylactic treatment to maintain the isolation of cases of true diphtheria as long as any Loeffler bacilli are found. Accordingly, at the Willard Parker Hospital the resident staff make cultures from the throats of convalescents at frequent, almost daily, intervals after the disappearance of the membrane, in order to discharge the patient as cured only when several days have elapsed

without the finding of diphtheria bacilli in the cultures. Although this adds a great many days of hospital care to individual cases, it prevents the carrying of infection to healthy friends outside. It would be well if the laity could understand that a case of diphtheria should remain isolated until the absence of diphtheria bacilli in the throat has been proved.

Now as to the patient's friends, nurses, and physician, looked upon as a source of contagion. It may be taken for granted that in everything the patient's clothes, skin, hair, fingers, mouth, etc., touch, there is a strong probability of some bacilli being caught, for the patient contaminates his own body, together with his surroundings, with pseudo-membrane and discharges, which dry on his person and surroundings, and are thus taken up by others. All the more is this true because the little people whom diphtheria has chosen for its own, are not old enough to be the subjects of active personal cleanliness. It follows, therefore, that those engaged in nursing the patient should be, as far as possible, isolated from other people. The physician above all people should guard himself from contamination, if not for his own sake, for that of his *clientèle* and friends. At the Willard Parker Hospital it is the custom before entering the diphtheria wards, for the physician to envelop himself in a muslin gown with a hood over his head. This gown has been previously sterilized by baking; it is long enough to reach almost down to the heels of the shoes—these latter are covered with ordinary rubbers. Thus clothed, the physician enters the hospital ward. I have made it a rule in private practice, in lieu of the gown, to have plenty of towels to facilitate the handling of the patient. His body is not touched by my bare hand without the intervention of a clean towel; whether it be the head or the face or the wrist or chest of the patient that must be handled, it should always be with the intervention of a towel. A towel should be placed upon the chair upon which the doctor sits, and

another one folded upon his lap. All instruments used for examining the throat and nose should be previously and afterward sterilized, and a solution of creolin or bichloride of mercury used to wash the hands immediately after leaving the apartment. As far as the friends are concerned, the best of all precautions is isolation. That this cannot be complete in the present state of society, and in consideration of the enormous frequency of the disease, goes without saying. But the nearer we approach the ideal isolation, even with regard to mild cases of diphtheria, the less frequent will be the disease. It is not at all rare to find children suffering from true diphtheria, without constitutional symptoms and but little angina, sent to school, where they infect healthy children. To guard against this, children and adults complaining of the slightest throat lesions should be isolated.

The room in which the patient lies should be isolated, not only from the rest of the household, but measures should be taken to keep strangers from coming in contact with the apartment in which the patient is, and also with those engaged in waiting upon him. Above all things should cases of diphtheria be carefully isolated when they occur in the tenement districts, although perfect isolation is here practically impossible. It is needless to say that diphtheria occurring in living apartments, in connection with tenement-factories, candy-stores, butcher-shops, milk-shops, and bakeries, is especially dangerous, owing to the broad channels of infection of the community which these occupations open.

A third source of infection is the fact that the furniture, utensils, clothes, and even the walls of the rooms,—in fact, all the surroundings of the patient, become contaminated with discharges and sputa containing Loeffler bacilli. Here the question arises, How shall all these various objects be made free from the bacteria of diphtheria? Some articles cannot be so disinfected, and such should, if

possible, be destroyed. The walls should be painted, old wall-paper should be removed and replaced by new; floors scrubbed with carbolized water in a one per cent. solution, or mercuric chloride solution of 1 to 4,000. All articles, such as bedding, blankets, mattresses, etc., should be subjected to dry heat at 100° C., in other words, baked. Steam at 100° C. will, of course, destroy all bacilli, but probably also the furniture. We are forced, therefore, to rely upon chemical bactericides, such as vapor of burning sulphur and chlorine gas.

As to the further question now attracting so much attention, as to whether it is possible to render animals immune to the toxic products of the bacillus of true diphtheria, although properly belonging to the prophylactic treatment of diphtheria, it will be best to discuss this under the head of the antitoxin treatment, and thus avoid repetition of the same facts.

Treatment of the Local Manifestations of True Diphtheria.—The relative importance of the local as compared with the constitutional treatment of true diphtheria depends upon the site of the local manifestation, and, to a certain extent, the stage in which the case comes under treatment. It is evident, for instance, that in a case of diphtheritic croup, with threatening death from dyspnœa, due to occlusion of the larynx, the importance of relieving the stricture by intubation or tracheotomy for the time overshadows the necessity of proper constitutional remedies to counteract the poison of the disease. Again, many observers believe, among them Loeffler himself, that if the site of the primary lodgment of the Loeffler bacilli is locally treated sufficiently early to destroy these before the toxic elements which they produce have affected the system, that systematic remedies become superfluous, so that under any circumstances the treatment of the primary local lesion of diphtheria is an extremely important one. A consideration of cases of diphtheria as they occur clinically will

teach us that the local treatment has for its object: 1st, the removal of the toxalbumins which are the result of the activity of the Loeffler bacilli; 2d, the destruction of these bacilli; 3d, the hastening of the separation and expulsion of the false membrane; and 4th, the relief of fatal dyspnœa, resulting from obstruction of the larynx and trachea by false membrane and other diphtheritic products.

Fortunately, the usual location of the primary lesion of diphtheria—the throat and nose—is so easily inspected that a diagnosis of a commencing diphtheria can often be made early enough to render the local treatment an important factor in the cure of the disease. The remedies and their mode of application will depend to a certain extent upon the situation of the pseudo-membrane; thus toxic products, bacilli, and false membrane situated in the larynx cannot be attacked by fluid solutions, but rather, if at all, by vaporized remedial agents, such as calomel fumigation or turpentine inhalation, for instance; while the pharynx, tonsils, buccal and nasal cavities are open to treatment with solid or fluid agents. It would be futile to discuss all the methods and agents used in the local treatment of pharyngeal and nasal diphtheria, and I shall mention only those procedures which I have found effective both in private and hospital practice. In the first place, the washing away of the toxic agents, the false membrane, and other diphtheritic products can be best accomplished by the mechanical action of fluid solutions. In these solutions we may dissolve agents that are more or less germicidal, such as the bichloride of mercury, boracic acid, carbolic acid, peroxide of hydrogen, salt, etc. The ideal solution will be that one which can be used in quantities sufficiently large to clean and wash off all of the deposits that cover the mucous membrane without being too adherent to it, and without injury to the mucous membrane or to the system, if swallowed; such an ideal solution should, furthermore, if possible, have the power of destroying the Loeffler

bacilli and its toxic products. It is evident that an ordinary bland, neutral salt solution, at a temperature of 85° F. can be used in unlimited quantities until all the loose diphtheritic products have been washed away; such a solution, however, has no germicidal properties. There are those who do not believe that anything is gained by the germicidal action of the solution used for douching the throat and nose of these patients. Thus a great many cases are treated in the Willard Parker Hospital locally by irrigation of neutral salt solutions with very good results. To test the question of the advisability of using antiseptic and germicidal agents in the irrigating solutions for the throat and nose at the Willard Parker Hospital, a series of cases were subjected to local irrigation with warm salt solutions every one to three hours; a second series were subjected to this treatment, and, in addition, were sprayed every three hours with peroxide of hydrogen, in the shape of pyrozone, recommended by Dr. Northrup, varying in strength from five to twenty-five per cent., according to the severity of the case; and a third series of cases were subjected to irrigation by a 1 to 3,000 or 4,000 solution of the bichloride of mercury.

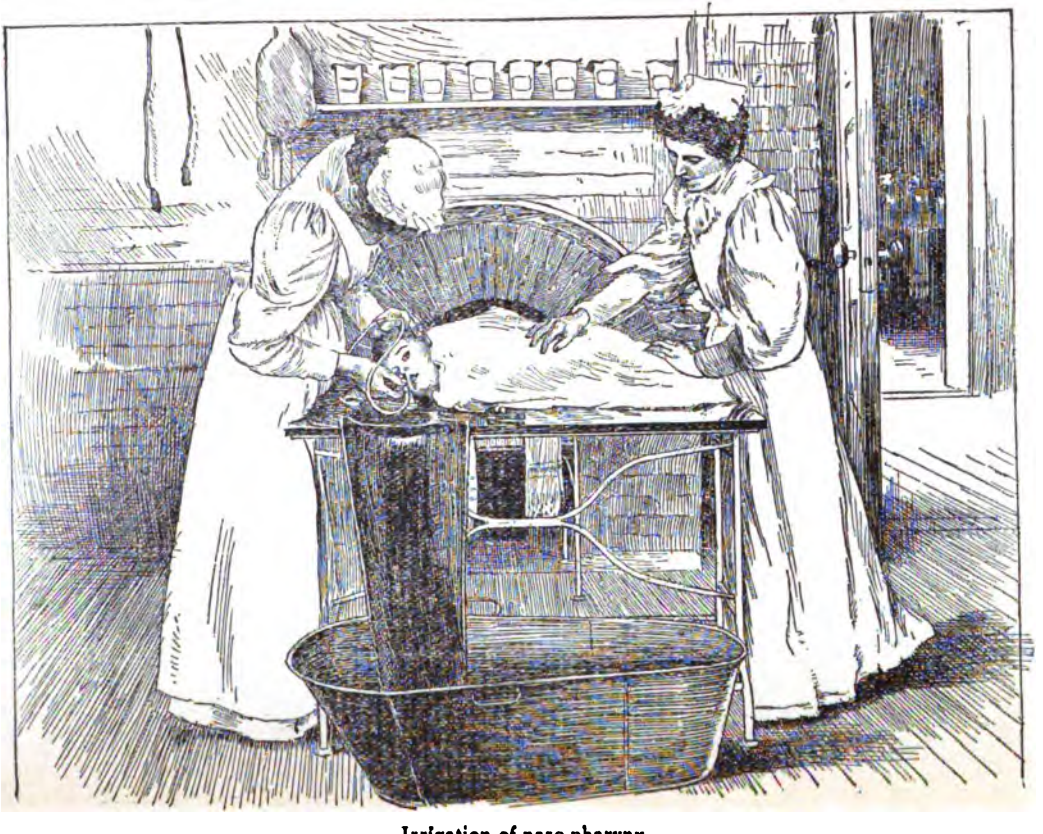
It was found that while the average number of days in which membrane persisted in the throats of those treated by salt-water irrigation was only six to nine days, that under bichloride irrigation the average duration was seven to nine days, while with salt water and pyrozone it was nine to sixteen days. The bacilli, however, persisted in the salt-water cases for 10.2 days, while in the bichloride cases they disappeared 7.4 days after the disappearance of the membrane, and in the pyrozone and salt-water cases, 9.6 days. It would appear, therefore, that a mechanical washing away of the diphtheritic products is best accomplished by the warm salt-water irrigation, probably because being harmless it can be used in much larger quantities than a germicidal solution, such as the mercu-

rial sublimate; but the bacilli, and probably what is more important, the toxic products, disappear most rapidly under a bland germicide like corrosive sublimate, or in my experience what is equally good, a solution of boracic acid, 1 to 30, or a tablespoonful to a pint of water. The latter solution I use without salt water, it being sufficiently mild to use in large quantities. If the mercuric bichloride is preferred on account of its stronger germicidal action, the salt solution should be used for the mechanical work of thoroughly irrigating the throat and nose, and this should be followed by the solution of mercuric bichloride. Of course when the latter is used, should symptoms of mercurial poisoning occur, which I saw once at the Willard Parker Hospital during the summer, then the bichloride should be stopped and the boracic acid substituted.

Now as to the frequency with which irrigation should be done. This varies with the severity of the case. When the nose and throat fill up rapidly with the secretions, the irrigation should be at shorter intervals until the improvement in the amount of discharges renders less frequent irrigation equally effective. We must not forget, however, that irrigations, even if properly performed, are accompanied by more or less disturbance, and sometimes even violent resistance on the part of the patient, and that the strength of the patient is thus apt to become exhausted. It is of the utmost importance, therefore, if we would husband the vital energies of the patient and avoid heart failure, which is one of the chief dangers of diphtheria, that these irrigations be performed at intervals sufficiently far apart not to weaken the patient and yet sufficiently often to keep the nose and pharynx properly cleaned. Rarely do I think it necessary to subject the patients to irrigation more frequently than once in two hours, generally only once in three hours; both nose and throat being irrigated.

The method that disturbs the patient the least should be the one selected; that I believe consists in the use of the fountain syringe. The patient, if a child, should be wrapped in a sheet, so as to pinion the arms, and then placed recumbent on the side upon a table or bed, a rubber sheet passing from the patient to a receptacle on the floor; the syringe is hung several feet above the level of the patient, and the nozzle, with a single opening, passed gently into the upper nostril as the patient lies on the side, parallel to the floor of the nasal cavity. The solution will wash out the nasal cavity and nasopharynx, pass out through the lower nostril and through the mouth; the stream should be controlled so as to irritate the patient as little as possible. The lower nostril is then irrigated; than the patient remaining in the same position, and a large nozzle having been substituted, generally the larger rectal tube that goes with the syringe, the throat and mouth are irrigated, care being taken that the nozzle of the syringe is inserted well over the dorsum of the tongue, so that the solution may reach the pharynx.

The advantages of this form of douching are, 1st, the patient is recumbent, and thus the strength is not wasted; 2d, sufficiently large quantities of the solution can be used without such interruption as is necessitated by the piston syringe; 3d, far less violence is necessary than in the use of the piston syringe; 4th, this irrigation can be accomplished with the least assistance, one nurse being generally sufficient to manage the tube and patient; 5th, there is the minimum of chance of washing bacilli into the larynx in cases in which the larynx is not previously affected. Occasionally where the plugs in the nose and posterior nares are so thick and firmly adherent that this douching does not dislodge them, the force of a piston syringe, with a soft rubber tip, so as not to injure the mucous membrane of the nose, may be employed; finally, and this is rarely necessary, a long probe, carefully



Irrigation of naso-pharynx.

guarded with cotton, may be passed through the nostrils, to be immediately followed by irrigation, which generally clears out the nostrils and keeps them open for the future. Swabbing, pencilling, and scraping the throat and nose are not useless, but are actually excessively injurious, owing to the abrasions which result upon the mucous membranes, each of these abrasions becoming immediately infected and diphtheritic.

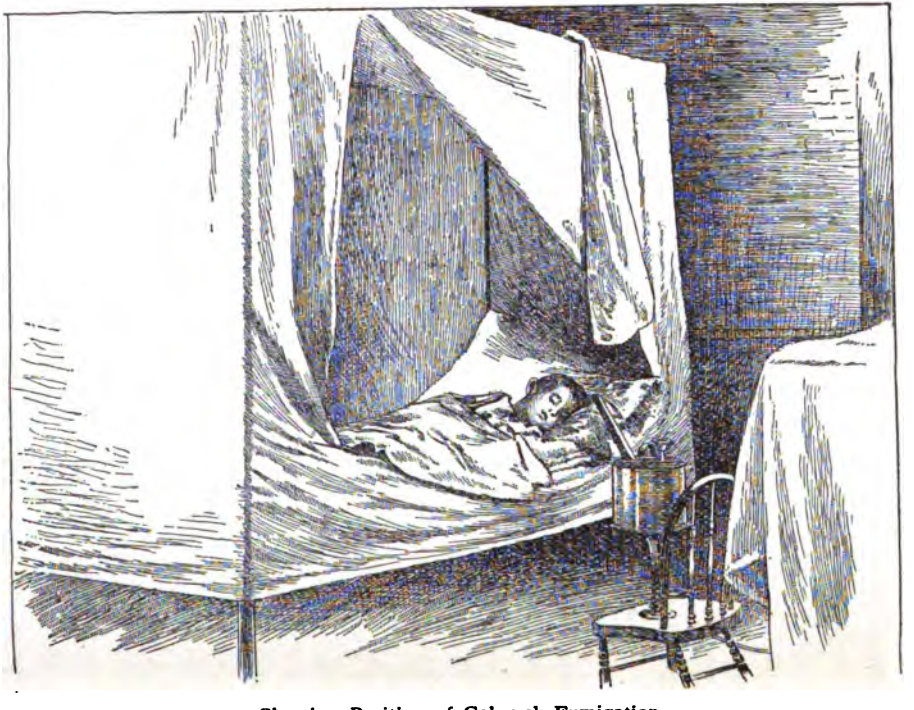
I have occasionally blown small quantities of iodoform into the pharynx in cases where the appearance of the throat was extremely septic in character. I have never observed in such cases any symptom of iodoform poisoning. Peroxide of hydrogen I have used in weak and strong solution, often the commercial solution of sixteen to seventeen per cent. by volume, pure; I have employed it as a spray and injected it with a glass syringe both in the throat and nose, and while theoretically the germicidal action of the peroxide of hydrogen should render it extremely useful in this disease, I have not found that the cases in which I have used it have done satisfactorily; the reason is that the preparation is extremely irritant, causing inflammation in the mucous membrane, with which it comes in contact when in strong solution, and this inflamed mucous membrane is apt to become diphtheritic. The weaker solutions, although non-irritant, are not strong enough to be germicidal nor bland enough for the mechanical washing effect.

I must not neglect here to mention a local treatment for diphtheria recently proposed by Loeffler, and for which he makes enthusiastic claims. In the *Deutsche medicinische Wochenschrift*, of October 18th, 1894, Loeffler reports 71 patients treated in private practice by this new method, with 71 recoveries, all these cases having begun treatment early in the disease. Of 30 patients seen in hospital practice 5 died. All of these were seen in the second stage of the disease. The treat-

ment is entirely local, and has for its object the destruction of the bacilli before their toxic products have been absorbed into the circulation. He applies to the diphtheritic mucous membrane of the pharynx or nose, as the case may be, the following solution: Alcohol, 60 volumes; toluol, 36 volumes; liq. ferri sesquichlorati, 4 volumes. To alleviate the pain which the application of such a solution to the throat entails, he adds 20 volumes of menthol. He finds that this solution, when brought in contact with thick culture layers of bacilli, in the laboratory, kills them in five seconds. The method of application consists in cleaning the affected mucous membrane with pledgets of cotton, then applying similar pledgets, thoroughly saturated with the solution, against the affected areas for the space of ten seconds. This should be repeated about every three hours at first, until the pulse becomes slower and the temperature falls; then only three times daily until membrane and bacilli disappear. The high authority of Loeffler's name would sanction the faithful trial of this remedy, but it is evidently open to the objection that it is scarcely applicable to cases of true diphtheritic croup, since here we cannot make these applications with any degree of completeness, especially where the process extends into the trachea.

We come next to consider the local treatment of laryngeal true diphtheria, or membranous croup; here, as already hinted, the conditions are such as almost to preclude the use of fluid local remedies, for the reason that the mucous membrane of the larynx is not readily approachable; 2d, because fluid applications are apt to pass down through the trachea into the bronchi, and thus give rise to a pneumonia. Again, we have other conditions to treat in laryngeal diphtheria than in diphtheria of the pharynx and nose. For here the most prominent and disastrous effect of the disease is the dyspnœa resulting from, and varying with, the more or less perfect occlusion of the glottis and

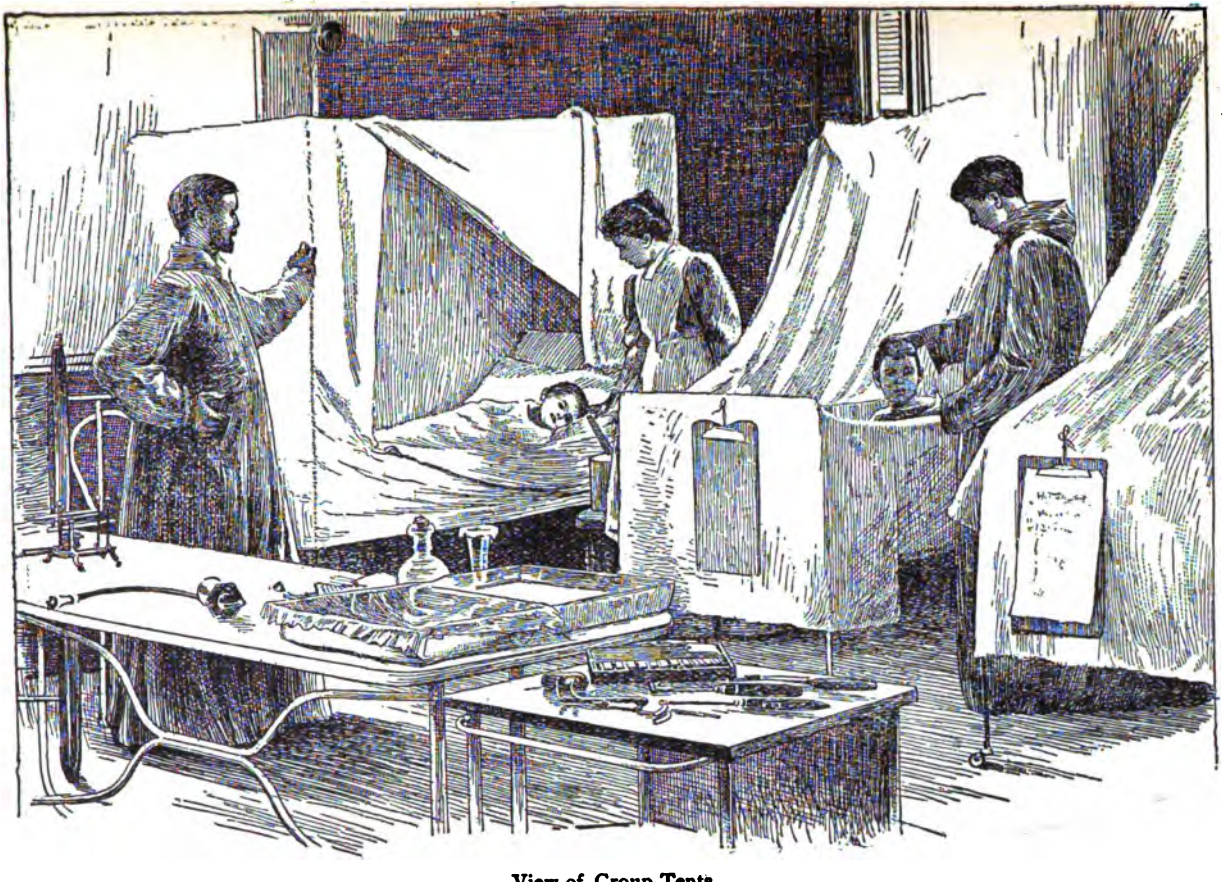
the lumen of the larynx and trachea; furthermore, the inhalation of the septic diphtheritic products located in the respiratory tract is apt to give rise to diphtheritic or septic bronchitis and pneumonia. The local treatment, then, here has for its object, 1st, the prevention and removal of the swelling and false membrane affecting the mucous lining of the glottis, larynx, and trachea, and narrowing and occluding the calibre of the respiratory tract; 2d, the destruction of the bacilli and their toxic product, in order to prevent the extension of the diphtheritic process into the bronchi and air-cells. It is evident that the form in which remedial applications can be best applied to this region is in the form of vapor or gas. Every inspiratory effort should, if possible, result in the inhalation over the diseased mucous membrane of a curative vapor. There are many of these, designed to produce various effects. Thus it has been thought that inhalation of steam reduces the swelling of the mucous membrane of the respiratory tract, to which much of the narrowing and mechanical occlusion of the tract is due; furthermore, these inhalations are supposed to hasten the separation and expulsion of the false membrane located in the larynx and trachea. It has always been a question in my mind whether the inhalation of the vapor of water has any such effect, for in cases in which there is false membrane and swelling in the pharynx, which we can observe, I have not noted any such effect from the inhalation of steam alone; but there is no doubt that such steam inhalations are extremely agreeable to the little sufferer, especially if they are warm and long continued. Steam inhalations in the treatment of croup were first recommended by Dr. L. H. Sayre, of this city, about thirty years ago. I must lay some stress upon the importance of having the steam warm, which is never the case in the ordinary steam spray apparatus known as the steam atomizer. In the Willard Parker Hospital there is in use an ordinary kettle, made of tin,



Showing Position of Calomel Fumigation.

with copper bottom, placed upon a tripod, with a Bunsen burner under it; this kettle has a long nozzle, which delivers the steam about on a level with the child lying in a cot. A similar kettle, though not quite so large, can be bought at the stores and used in private practice. While ordinary steam gives the emollient effect of a poultice to the inflamed mucous membrane, it has been sought to use the steam as a vehicle for the application of germicidal agents to the bacteria located at the site of the disease in the larynx. I have thus used the oil of turpentine, creosote, and carbolic acid. With all of these agents I think some effect upon the dyspnœa is obtained. I have been led to believe that the effect of vapor of water thus medicated is better than that of vapor of water alone. The same apparatus is used as for ordinary steam, a small quantity of the turpentine or creosote or carbolic acid being poured upon the surface of the boiling water. These agents stimulate the patient to cough, and much of their beneficial effect should be ascribed to this circumstance; as to any germicidal action, the use of vapor of water, to which carbolic acid or creosote has been added in sufficient strength to produce such an effect, is irritating to the lungs and apt to give rise to pneumonia and bronchitis. I have yet to mention, however, one of the most important local applications of a vaporized remedy in croup, and that is the inhalation of the fumes of burning calomel. That mercury, when given internally in diphtheria, and particularly diphtheritic croup, is of the greatest value, an experience extending over seven or eight years has convinced me; and it appears to me that the beneficial effects of the inhalation of calomel fumes in cases of diphtheritic croup depends to a great extent upon the constitutional action of the mercurial vapor absorbed by the mouth, throat, and lungs. Nothnagel, in his work on therapeutics, shows that the action of inunctions of mercury cannot depend upon absorption through the skin; it depends upon

the fact that such inunction, owing to the heat of the body, vaporizes the mercury; this mercurial vapor is directed by the clothing to the mouth, and thus inhaled and absorbed; how much more likely is it that sublimed calomel is absorbed by the vast absorbing surface represented by the air-cells and mucous membrane of the bronchi and trachea! I believe, then, that the only way in which calomel causes a diminution in the swelling of the affected portions of the respiratory tract, and hastens the loosening and liquefaction of the false membrane, is by being absorbed when thus locally applied, and producing the constitutional effects of the mercury. It is possible that it has some effect as a germicide locally, and the strongly irritant quality of the vapor certainly causes cough; but these results are only secondary, and certainly are inadequate to explain the excellent effect of the calomel fumigation in diminishing dyspnœa and softening the cough of cases of membranous croup. The calomel should be burned under a tent formed over the bed of the patient. The tent should be as complete as possible, and not too large, so as not to require too much calomel for fumigation. The calomel should be sublimed upon a tin plate over an alcohol lamp. I present herewith a calomel sublimer, which I consider excellent, used at the Willard Parker Hospital. The quantity that should be burned is about ten grains every two or three hours, depending upon the severity of the dyspnœa, until the patient is relieved; here, as from the internal administration of mercury, constitutional effects, such as diarrhœa, salivation, etc., should be looked out for, and the procedure guided accordingly. Salivation, however, is very rare in young children. I have neglected to state that the tent over the cot of these little croup patients is an important factor in the local treatment of croup, for even steam inhalations can only be done effectively if the patient is made to breathe the air of a confined space filled with steam. There are,



View of Croup Tents.

of course, a vast number of other local remedial medicinal agents for diphtheritic laryngitis, but those I have mentioned are the ones with which I have had the most experience.

Finally, we have for the relief of the dyspnœa in diphtheritic laryngitis which threatens to become fatal, two surgical procedures, the one ancient—tracheotomy; the second recent, modern, and American—the method that the world owes to the genius of O'Dwyer, intubation. It is customary in these days to contrast these two methods of operating, with the view of finding which should be preferred. A comparison of the two procedures is as preposterous as it would be to discuss whether it is better to extract a foreign body from the œsophagus, or perform œsophagotomy or gastrotomy. In cases in which the milder operation is sufficient, certainly the severer and more fatal operation should never be performed. The object of the operative procedure being to relieve dyspnœa, that operation should be selected which can do it most effectively and most safely—by safely we mean ultimate as well as immediate safety. It will be conceded without argument that even in the hands of the most skilful surgeon tracheotomy is by far a more serious operation than intubation; but to enumerate all the advantages of the latter operation is hardly necessary when it is remembered that the percentages of recovery from intubation, taking all the cases, are vastly better than those from tracheotomy. Many parents who will subject their child suffering from croup to intubation would refuse tracheotomy; superior surgical skill is not requisite to its performance. There is little or no danger from the operation, if we exclude the possibility of pushing down false membrane in front of the tube, which occurs rather rarely. The after-treatment is simple, and does not absolutely require trained nurses for its proper fulfilment. There is no wound to heal after the tube is removed. The objec-

The Method of Intubating.

tion to intubation is the difficulty in feeding patients with a tube in the larynx; even small quantities of liquids when swallowed pass into the trachea instead of the œsophagus, and give rise to a pneumonia, due to the lodgment of liquids in the smaller bronchi and air-vesicles. And yet, taking all the advantages and disadvantages into consideration, I think we may formulate the rule that in cases in which the dyspnœa can be relieved by intubation, this operation should be performed. If subsequently the tube no longer relieves the dyspnœa, then a larger tube may be tried, or tracheotomy may be performed. I should then limit the performance of tracheotomy to those cases of membranous croup in which the tube does not relieve the dyspnœa primarily, or secondarily, when the disease having extended below the level of the tube, the latter no longer is effective.

It is hardly within the province of this paper to discuss the method of the performance of these two operations. A few important points in management of intubation cases cannot be too often repeated.

1. I have found the best tubes those most recently manufactured, with the bulbous ends.

2. In inserting the tube, the gag should be placed well back between the last molar teeth.

3. The tube should be thoroughly sterilized before being inserted, by dipping in boiling water. I think that frequently the extension of the disease below the level of the tube is due to the use of a dirty tube.

4. Liquids should be fed the patients in small quantities, the patient lying on the back, on the lap of the nurse, with the head thrown back, thus bringing the back of the pharynx on a lower level than the larnyx.

5. If the baby be a nursing baby it should be fed in that way even when intubated. I have been accustomed, in older children, where in spite of all efforts neither food nor drink can be swallowed, to remove the tube every

second day, sometimes even every day after the second day, feed the patient, and then replace the tube; this, however, is very rarely necessary. I generally order ice for the thirst instead of water; ice-cream, condensed milk mixed with ordinary milk in equal parts, thick broth, the yolk of eggs; these are the chief articles of diet while the tube is *in situ*. Before extracting the tube, the blades of the extractor should be examined to see that they are firm. I have seen one of them break off just as the extractor had been placed in the mouth. The patient should be kept under a tent filled with warm steam while the tube is in the larynx.

A word as to emetics. I do not think it ever necessary to administer emetics, except at the beginning of the disease, and then chiefly as a means of differential diagnosis between true and false croup. The latter cases are cured by a sharp emetic, and little other treatment, while true croup is temporarily relieved, but soon the membrane reforms, the swelling returns, and the patient is as bad as ever. To relieve dyspnœa in true diphtheritic laryngitis, emesis can be but a temporary expedient. The emetic I prefer in all of these cases is the yellow subsulphate of mercury.

Constitutional Treatment.—I do not think that any one will question that by far the most frequent cause of death from diphtheria, except when it is located in the larynx, is the pathogenic action of the toxin formed under the influence of the Loeffler bacillus. This toxin is absorbed into the general circulation, and gives rise to the familiar manifestations of toxic diphtheria, such as fever, swelling of the lymphatics, disturbance of the heart's action, manifested by rapid and feeble pulse, and sometimes heart failure, inflammation of the kidneys, lungs, brain, and its meninges, the formation of abscesses in different portions of the body, and in the later stages of the disease, paralysis due to toxæmia and inflammations of the peripheral and

central nervous system. All of these are chiefly the result, in a general way, of the absorption of the toxic material into the blood. Even where the inflammatory lesions are due to the direct extension of the diphtheritic process, as in diphtheritic bronchitis, and pneumonia, inflammation of the middle ear and mastoid sinus, the absorption of toxins into the blood adds greatly to the danger. The constitutional treatment of diphtheria, then, is by far the most important of any that we have considered. Such constitutional remedies at present in use in this disease are either symptomatic, empirical, or rational. The symptomatic remedies are those whose use is intended to counteract the evil symptoms of the action of the toxin. Thus digitalis, strychnine, caffein, etc., are intended to obviate and remove the signs of weak heart's action; empirical remedies are such as have been found to exert a favorable influence upon the constitutional manifestations of diphtheria, by experience, without a really adequate explanation of their action being known. In this group I would class the most important remedies that we have against the action of the diphtheritic toxin, bichloride of mercury and tincture of the chloride of iron, although some might class both of these among the so-called rational remedies for diphtheria. Finally, we have the most recent remedy for diphtheria toxin, the so-called antitoxin treatment, which, if all that has been claimed for it turns out to be true, will, as is always the case with rational remedies, replace the empirical absolutely.

In considering the symptomatic remedies for the constitutional symptoms produced by diphtheria toxin, one of the most important symptoms which comes up for treatment is the fever. This varies in amount, but not in proportion to the gravity of the disease. Very septic cases of diphtheria may have little or no fever, while mild cases sometimes "run" high temperatures. At the beginning of the disease the fever is probably due to the local inflamma-

tion of the tonsils and not to the absorption of toxins. This fever will subside within the first twenty-four hours, so that a patient with a high temperature at the outset may be bright and smiling on the following morning, while the throat is found to be full of membrane. The treatment of the fever of diphtheria should never be by antipyretics. These drugs weaken the activity of the heart. Hydrotherapy only should be used. This includes cold compresses about the neck for their local action, sponging with a mixture of five parts cold water and one part alcohol, and Ziemssen's baths. The temperature does not require any treatment if it be below 102° F., and cold sponging in a warm room, followed by gentle rubbing with a dry towel, is to be preferred to baths. The frequency of the sponging or baths will depend upon the temperature, and should be suspended entirely when the temperature has fallen to 101° F. In this connection it is well to remember that a small amount of fever has a sustaining rather than a weakening effect upon the pulse. The sustaining of the weak heart of diphtheria is the most important symptomatic indication in the treatment of this disease. It should be enforced in every case before the appearance of the signs of a failing heart. In diphtheria as little work should be placed upon the heart as possible. Such patients should not sit up, but maintain a recumbent position. The patient should be kept lying in bed as long as membrane is present, however slight the symptoms may be. Severe cases should be kept in bed for weeks after the membrane has disappeared. All local applications made to the throat should be so made with the least possible physical disturbances, and, when possible, with the patient in the recumbent position. In an excessively excitable patient I frequently prefer to suspend for a time the local treatment of the disease rather than exhaust the patient. Indeed, the frequency of local applications, such as irrigation, should depend partly upon

Proper Method of Feeding Intubated Patient.

the severity of the local disease, and partly upon the strength and resistance of the patient to the local treatment. Stimulants and diet should be such as will best maintain the strength of the heart. Thus I give whiskey, brandy, and other alcoholics, together with a fluid diet, milk beef-broth and beef-juice, eggs, etc., avoiding all forms of diet and stimulation which cause vomiting. In severely sick children, where the patient refuses food and stimulants, rectal feeding must be employed; I have no experience in feeding such patients with the stomach-tube; the objection to this is the struggling of the patient when the tube is passed, although some cases, I have no doubt, will do better fed in this way than by the rectum. As much of the alcoholic and fluid diet as possible should be given at frequent intervals and in small quantities at a time. Of the alcoholic stimulants, whiskey is to be preferred, diluted with milk or water.

Of stimulant drugs, strychnine should be given at the very beginning, in order that its maximum physiological effect may be attained about the time when the heart begins to flag. It is best to give considerable doses, frequently repeated; to a child two years of age $\frac{1}{120}$ of a grain every two or three hours if necessary. Should symptoms of heart failure occur, hypodermatic injections of strychnine, nitroglycerin, and caffeine, in the shape of the double salicylate of soda and caffeine, are indicated. Counter irritation of the heart is also indicated. Digitalis does not act well, in my experience, in the heart failure of diphtheria.

There has always been in this disease the desire, on the part of our profession, to discover a specific remedy that is common to all frequently fatal diseases; such specific remedies have generally been advocated upon an empirical basis. The empirical drugs in treating diphtheria with which I have had experience, are the tincture of the

chloride of iron, and the bichloride of mercury. I do not think that the use of either of these is really empirical in the treatment of diphtheria. The tincture of the chloride of iron in large doses, fifteen to sixty drops every hour or two, undiluted and only mixed with a little glycerin, certainly has some local effect upon the throat as it is swallowed.

The recommendation by Loeffler of the perchloride of iron solution as a local specific against diphtheria, of which I have already spoken, would indicate its local value; when swallowed, if absorbed by the stomach, it would appear to act as a blood stimulant, supporting the heart through a wasting disease. This remedy in large doses (and only in such doses has it any possible effect), is very popular, having the sanction of the highest authority. I must protest against the administration of this drug in small doses, at long intervals, unless the physician intends it to act simply as a placebo.

While I consider the iron treatment as given above adequate in milder cases of diphtheria, I do not rely upon it exclusively in severer cases, but have much more confidence in the use of large and frequently repeated doses of corrosive sublimate. This treatment of diphtheria and diphtheritic croup was first introduced into this city by Dr. A. Jacobi, and unless the antitoxin treatment should prove all that is claimed for it abroad, I have considered it by far the most valuable remedial agent given to us in diphtheria. Its good effects are as remarkable in the treatment of diphtheritic croup as in the treatment of general diphtheria. In this respect it differs widely from the iron treatment; I consider the latter absolutely valueless in cases of uncomplicated membranous croup, with no septic manifestations, while the mercurial treatment is to the medical treatment of true croup what intubation is to the operative treatment of the same disease.

Fully half the cases of membranous croup, if properly

treated sufficiently early by corrosive sublimate, will not require intubation of the larnyx, and those that have been intubated do far better if they have been under the influence of the mercurial sublimate solution before the intubation, even if this has not succeeded in relieving them without the operation. I have already stated that the most valuable local remedial agent in croup is calomel fumigation, and I am convinced that this is of value only through the absorption of the mercury by the mucous membrane of the bronchi and through the air-vesicles. In other words, calomel fumigation only hastens the saturation of the blood with mercury. As to why the internal administration of the bichloride of mercury is of such value in diphtheria I cannot say.

Mercury certainly is excreted by the mucous membrane of the respiratory tract. It increases the secretion of the mucous follicles as it does that of saliva, and probably in this way accelerates the loosening and separation of the false membrane. Upon pharyngeal diphtheria it can possibly exert a slight local effect, of an antiseptic character, as the hourly or half-hourly dose is swallowed. As to whether it exerts a destructive action upon the diphtheria toxins in the blood is very doubtful. Nevertheless, clinically this is by far the most powerful curative medicinal agent in the treatment of diphtheria. It should be given in comparatively large doses frequently repeated. I give to a baby a year old $\frac{1}{60}$ grain every half-hour or hour, and this can be continued for several days if necessary. Salivation in these little patients is very rare.

The occurrence of mercurial diarrhœa is the chief indication for the suspension of the remedy. It is rarely or never vomited. It should be given alone, dissolved in distilled water; never given in a mixture with iron, never mixed with an organic drug or syrup. If albumin be present in the urine when the treatment is begun, its increase should signify a diminution of the drug; if no

albumin is present in the urine, its appearance means a suspension or diminution of the remedy. The iron treatment is also contraindicated if the albumin in the urine of the patient is increasing.

We now come to the consideration of the most remarkable method of treatment that has ever been proposed for true diphtheria, and the only one to which the term rational can be applied, namely, the *serum therapy* as applied to this disease, that is to say, the antitoxin treatment of diphtheria.

By the methods hitherto advocated, diphtheria has been treated with considerable success; indeed, with rather more success than they have had in Germany or France, judging from statistics recently published. And yet diphtheria has been a frightful scourge to very young children, killing larger numbers of them than perhaps any other acute infectious disease. With the view of eradicating this scourge, bacteriologists and clinicians turned their attention to discovering some method by which human beings could be rendered immune to attacks of true diphtheria, and in a general way to attacks of other acute infectious diseases. While methods were discovered by which animals sensitive to diphtheria could be rendered immune to the action of the Loeffler bacillus and its toxin, it was found that similar principles could be applied, in the treatment of diphtheria which had already developed in animals, with success.

As early as 1868, an Italian had vaccinated patients suffering from diphtheria with diphtheritic virus, and under this method of treatment lost but one case out of fifteen treated. Bagola, of Sienna, using a like method of treatment, had similar results in twenty-nine cases. One can readily see the objections, however, to a method of treatment, or of procuring immunity which would necessitate the inoculation of human beings with active poison like diphtheria virus, or the infection of cultures

of diphtheria bacilli into the blood of human beings with the object of rendering such persons immune; the reaction to the poison might be sufficiently great to cause a fatal diphtheria. Brieger and Fraenkel succeeded in rendering guinea-pigs immune against virulent cultures of the diphtheria bacillus by injecting bouillon cultures, three weeks old, of Loeffler bacilli; they sterilized these cultures by exposure for an hour to a temperature of 60° C. to 70 C. They injected from 10 to 20 c. c. subcutaneously. At the end of two weeks immunity was complete. It was found that all animals that are susceptible to diphtheria can be rendered immune to the disease by injecting into their bodies pure cultures of Loeffler bacilli in bouillon of gradually increasing virulence, beginning with the weakest cultures, such as will produce only the slightest reaction, and terminating when the most virulent cultures can be injected without producing any reaction either locally or generally. Such animals retain their immunity against diphtheria for a considerable period of time, varying in different animals, but not in any case permanently.

Behring should have the credit of first having discovered the fact that the blood serum of animals that have been thus rendered immune to diphtheria contains some substance which neutralizes the toxic agent contained in virulent cultures of Loeffler bacilli; the blood serum of such animals produced this wonderful effect when it was added to the filtered virulent cultures outside of the body, as well as when the cultures were injected into the living animal together with, immediately preceded or followed by, an injection of the blood serum of the immune animal. Indeed, he found that when the toxin is injected into a susceptible animal several hours before the injection of the blood serum of an immunized animal, the diphtheria toxin still neutralizes and the animal recovers. This protection and curative influence of the serum of animals rendered immune to diphtheria

was observed not only against the toxin but against injections of the pure cultures of the bacillus.

Upon the application of these discoveries to the human body depends the serum therapy of diphtheria in human beings, and from what we have said it will be seen that the following points are of importance in this connection:

1. How is the toxin, which is required for rendering animals immune, made and used?

2. How are animals rendered immune?

3. How is the blood serum of immune animals containing the antitoxin to produce its effects?

4. How is it to be applied, as to dose, frequency of application, etc., in the therapy and prevention of diphtheria?

5. What are the therapeutic benefits of the antitoxin treatment as compared with other forms of treatment of diphtheria?

1. The toxin for immunizing animals to diphtheria poison, that is, inducing in them, as it were, the diphtheria habit, is best made by cultivating extremely virulent bacilli in a current of moist air.[1] Alkaline bouillon containing two per cent. of peptone is placed in flat-bottomed vessels, supplied with a lateral tube for the admission of air above the level of the fluid. These solutions are sterilized and inoculated with very poisonous Loeffler bacilli, and heated in an oven to 37° C. When the culture has begun, a stream of air is admitted. After three or four weeks the culture is rich in toxins, and can be used for purposes of immunization of animals. These cultures are filtered through a porcelain filter. The clear fluid remaining is kept in the dark, in bottles well-corked, at ordinary temperatures. 0.10 c. c. of this toxin, thus kept, will kill a guinea-pig weighing 300 gm. in forty-eight to sixty hours.

2. How are animals rendered immune? The toxin above described is too strong for a first injection; it pro-

[1] Roux and Martine: Annales de l'Inst. Pasteur, 1894–viii–722.

duces a fatal diphtheria. It must therefore be diluted. Roux and Vaillard have used for this purpose iodine. Before injecting, they dilute the toxin with one quarter of its volume of Gram's solution, and after a few seconds inject it under the skin. The amount injected depends upon the size of the animal. An ordinary rabbit will stand a first injection of 0.5 c. c. of this solution of toxin and iodine. After a few days, the slight reaction having subsided, another injection is given in larger doses, and thus for a few weeks, the dose being gradually increased and the dilution with iodine diminished until the pure toxin is employed. The injection should be suspended whenever the animal shows a diminution in weight, for otherwise it may die. Some animals are more susceptible than others. Dogs thus immunized give a good serum. Sheep, goats, and cows are very apt to die during the process, and great care is required in these animals, to succeed in reaching the stage of immunity. For many reasons the horse has proved to be the best animal for the purpose. The horse has the advantage of not being very susceptible to the poison, 2.5 c. c. of the strong toxin often causing very little fever and very slight local œdema. When immunized, the horse furnishes a large amount of serum containing antitoxin, and the serum when injected under the human skin gives rise to no irritation and is entirely harmless. This animal is accordingly preferred by German and French experimenters for making antitoxin for therapeutic and prophylactic purposes. Vaillard reports as to the length of time necessary to immunize a horse to diphtheria. Beginning with an injection of ¼ c. c. of the iodine toxin mixture, he was able within two months and twenty days to inject 250 c. c. of the pure toxin without producing any local reaction or increase of temperature.

3. How does the blood serum of immune animals containing antitoxin produce its effects? Aronson[1] says that

[1] Berliner klinische Wochenschrift, 1895, xxxii–p. 356.

his experiments prove the antitoxin contained in the blood serum of immune animals directly destroys the toxin of diphtheria. This action occurs in the laboratory as well as in the living organism. Buchner and others, on the other hand, believe that there is no destructive action between the poison and the antitoxin.

If an animal receives a proper dose of serum, it resists a subsequent injection of toxin which would kill a guinea-pig of like weight not previously treated with the serum. The same result will follow if the toxin and antitoxin be injected together, or if the toxin be injected first, followed by the antitoxin at an interval of a few hours. In all these the guinea-pig will remain healthy, provided the proper proportion of the toxin and antitoxin be maintained. The quantity of serum necessary for a cure varies with the weight of the animal, the amount of toxin to be neutralized, and the time elapsed between the injection of the curative serum and the toxin. We know very little of the real nature of the "antitoxin" constituent of the antidiphtheritic serum.

4. As to the dose and method of application of the serum containing the antitoxin. The dose varies with the strength of the serum. Ehrlich has proposed a method for estimating this strength for any given sample of antitoxin serum. The unit of strength is 1-10 c. c. of such a serum, which so neutralizes 9-10 c. c. of normal toxin that the mixture injected under the skin of a guinea-pig produces no local manifestation. The blood serum used for therapy in human diphtheria is obtained by drawing the blood from the jugular vein. From this a perfectly clear serum is separated. To it is added a small amount of tri-cresol to keep it from decomposition. The immunizing power of the serum obtained from these animals is so great as to reach 100,000; that is to say, the serum will neutralize a dose of toxin that would kill the experimental animal, when it, the

serum, is administered in the weight of 1 to 100,000 of the weight of the animal.

Aronson[1] has succeeded recently in manufacturing very much more powerful antitoxin serum by using for his animals toxin obtained from very old and extraordinarily virulent diphtheria cultures. Behring's normal antitoxin, Roux's normal antitoxin, and Aronson's immunization solution are about of equal strength. Aronson's stronger solution is three times stronger than the immunization solution. From experiments with the serum on animals, we know that the dose of antitoxin serum for therapeutic purposes necessarily varies with the weight of the individual, the virulence of the diphtheritic manifestations, and the length of time that has elapsed since the patient has been affected with the first symptoms of the disease. Behring's, Roux's, and Aronson's immunization, or weaker solution, all have the effectiveness of 50,000 to 100,000. The dose is, for therapeutic purposes, 20 c. c. as a first injection, under the skin between the shoulder-blades or in the flank. If a bacteriological examination shows the presence of Loeffler bacilli, Roux gives 10 c. c. more in twenty-four hours. If no Loeffler bacilli are shown, the case receives no more antitoxin. These two injections, made under antiseptic precautions, generally suffice, although in some cases with high temperature and rapid and feeble pulse a third injection may be given. No evil results follow, either locally or generally, according to every observer who has reported his cases. Some observers have found a red rash following the injection of the antitoxin. In the cases treated at the Willard Parker Hospital, but two cases out of twenty showed a rash, of doubtful character, following the eruption. The antitoxin was used for purposes of rendering human subjects immune to diphtheria by Aronson, Roux, and others, but I have never seen it used for this purpose. It must not be forgotten that any immunity to diphtheria is

[1] Aronson: Internat. Klin. Rundschau, 1894.

only a temporary matter, for this is not a disease in which one or, at most, two attacks, grant immunity. Some patients have true diphtheria several times a year.

At the Willard Parker Hospital twenty cases, recently reported to a section of this Academy by the resident physician, were treated during the time of my service. The preparation used was Aronson's stronger antitoxin solution. The dose varied from 5 to 12 c. c.; the total number of injections given to all the twenty patients was thirty—an average of one and a half injections per patient—much less than Roux or the German authorities give.

5. We come finally to consider what benefits have been derived from the antitoxin treatment of diphtheria, and how they compare with results obtained by other methods of treatment. In the year 1892, there were 4,654 cases of diphtheria reported to the Bureau of Contagious Diseases in the city of New York—1,436 died. In 1891, there were 4,874 cases reported, of which 1,361 died, a death-rate for the two years of 29 1-3 per cent. During 1892, at the Willard Parker Hospital, there were treated for diphtheria 295 cases; died during the year, 79; death-rate, about 25.4 per cent.; but in children under five years of age, the death-rate was 44.55 per cent.; and from five to sixteen years, 11.7 per cent. only. The number of cases reported by physicians to the health department is, necessarily, far less than the number really occurring, for the mild cases are frequently not reported, so that the diphtheria death-rate for the city is probably much lower than that given above. The death-rate above given was under the customary treatment of diphtheria.

Curiously enough, the death-rate from diphtheria in the cities in Europe is very much larger than in ours. Martin and Chaillon report that in the Hôpital des Enfants Malades, there were treated, from 1890 to 1894, 3,971 children, of which 51.71 per cent. died—more than twice the death-rate at the Willard Parker Hospital for 1892; and probably

more than twice the death-rate in the city of New York at large for 1891–'92.

Similar statistics are obtainable from Berlin and Vienna; hence we must conclude either that the diphtheria in these cities is far more virulent than ours, or that our present methods of treating diphtheria are twice as efficacious as those hitherto pursued on the other side of the Atlantic.

Under antitoxin injections of the cases treated at the Hôpital des Enfants Malades from February 1st to July 24th, 1894, of 448 children, 109 died, or 24 1-3 per cent. At the same time, of 500 cases treated at the Hôpital Trousseau, 316 died, or 63 1-5 per cent.—a frightful death-rate for this disease. Deducting from the 448 cases 128 that were not true diphtheria, and 20 cases that were not treated, the death-rate is 78 out of 300 cases, or 26 per cent. The improved death-rate, then, from antitoxin treatment at Paris is not as low as our own death-rate under old forms of treatment.

Aronson, on the other hand, reports a reduction of mortality in cases of true diphtheria treated by antitoxin to 12 to 13 per cent. against 32 to 37 per cent., under the old methods of treatment. Katz reports 163 cases of true diphtheria, with a death-rate of 13 to 14 per cent., treated by antitoxin. All observers agree that of cases coming under treatment during the first day of the disease, all recover, and the further the disease has advanced before the beginning of the treatment, the less hope there is from the antitoxin treatment. Of the twenty cases treated at the Willard Parker Hospital, there were admitted and treatment began,—

Day of the Disease.	Number of Cases.		Died.	Per cent.
Third	7	of which	2	28⅔
Fourth	4	"	0	0
Fifth	5	"	1	20
Sixth	1	"	0	0
Seventh	2	"	1	50
Eighth	1	"	1	100

In all, 5 died, being 20 per cent. of the cases.

In view of the fact that the most reliable observers on the other side of the Atlantic report such a vast improvement in the death-rate from true diphtheria, it is proper for us to look to the serum therapy as applied to this disease, with a great deal of hope of benefit from its use in this country; but in view of the discrepancy between the results obtained by present methods of treatment in this country, as compared with results from the same treatment abroad, we must suspend judgment as to the value of antitoxin treatment in this country until we shall have proved by the treatment of thousands of cases as great a reduction in our present death-rate, relatively, as they have succeeded in obtaining in Europe. I have purposely not mentioned the complications of diphtheria in this paper, inasmuch as the subject is obviously too vast for consideration in one paper; it has been, rather, the aim to give a general outline of the treatment of diphtheria as followed in hospital and private practice.

DISCUSSION.

DR. W. H. PARK.—I first wish to say a few words about the bacteria shown on the screen. The difference between the pseudo- and the true diphtheritic bacilli was brought out a little more markedly on the screen than it is under the microscope. The pseudo-bacilli, while they stain rather more evenly, are very much like some of the less characteristic true diphtheritic bacilli, and it requires a careful examination in cultures to distinguish them.

In the very early stages of either diphtheria or pseudo-diphtheria, a culture is certainly necessary to establish the correct diagnosis, but later on, in a certain proportion of cases showing characteristic and well-marked appearances, I think the culture is unnecessary. In laryngeal cases, where no membrane is visible, I think it is safer, in cities at least, to regard them as cases of true diphtheria, whether the bacilli are found or not. Finding the bacilli in such a case means that they have been coughed up, or have become mixed with the mucus in the region of the pharnyx.

In the prevention of diphtheria, I think it is of the first importance to prevent convalescent cases from going out too soon, and also to prevent members of diphtheritic households from leaving as soon as the disease is discovered. Last summer an outbreak of diphtheria occurred at Fisher's Island, and immediately many of the summer residents left for their homes. Among these there was one family under my observation in which the disease developed after reaching home. In another case a nurse girl, ten days after complete convalescence, was placed in charge of some children, and one of the latter developed diphtheria and died. In another family in which diphtheria developed, two of the members who were not affected by the disease left immediately, going to a town in New Jersey, and as a result, several cases of diphtheria occurred there. So it is in this city; when a case occurs, some of the other children are often sent away and afterwards develop the disease and thus are the source of a new outbreak.

In the prevention of diphtheria, we have great hopes in the efficacy of antitoxin.

I believe that irrigation of the nose and throat with certain antiseptic solutions, such as bichloride, etc., has some effect, but most of us probably agree with the statement that cleansing, non-irritating solutions are the main thing. I am afraid we must abandon the hope of actually destroying the bacilli while acting on living tissues, because by doing that we also destroy the epithelium of the mucous membrane.

At the Willard Parker Hospital we have found that tracheotomy is often valuable in those cases in which the membrane is above the larnyx, as well as in the larnyx; that is, where the larnyx, pharnyx, tonsils, and nose are involved. In these cases it is so difficult to irrigate that tracheotomy is to be preferred.

Calomel has been spoken of as killing the bacilli. Experimentally, the fumes of calomel have very little effect on the germs, and children who receive frequent calomel fumigations seem to retain the bacilli in the mucus of their air-passages just as long as those who do not receive such fumigations.

As regards antitoxin, the papers lately have been so full of it that it is hardly necessary to refer to it here. In

the cases in which I have either used it or seen it used, it certainly seems to have proved of decided value, and I agree with the statement recently made in a German publication that this should not come under the same heading as tuberculin. The latter was more or less of a secret preparation; besides that, it was a distinctly dangerous substance. No one has reported any dangerous symptoms following the use of antitoxin, even after the large doses now given. I think very soon there will be sufficient antitoxin on the market, so that any one can obtain it. The Board of Health has purchased a number of horses, and from these we soon hope to obtain large supplies of the serum. Just one word more about the genuineness of these preparations. Pure, unadulterated horse serum cannot by its appearance be distinguished from that which contains the antitoxin, and I know that certain preparations are now being placed on the market which are absolutely worthless.

DR. J. E. WINTERS.—The paper of the evening was so comprehensive that a thorough discussion of it is of course out of the question. I shall therefore take up only a few of the points mentioned. It seems to me that a great deal of importance has been attached to the use of germicides, especially bichloride solution, in the local treatment of diphtheria. Experimentally it has been shown that a 1-1,000 solution of bichloride is required to destroy the bacilli, and furthermore, they must be kept in it for two hours. A solution of that strength is impracticable for irrigating purposes, especially in the nasal cases, where that method of treatment is most indicated. Even the weaker solutions of this drug, such as 1-4,000, cannot be regarded as harmless. They are much more prone to give rise to epistaxis than the neutral salt solution, and may produce serious constitutional effects. For irrigating purposes, therefore, bichloride is impracticable, as are also other germicides of this character. We come then to the use of simple cleansing solutions, and for this purpose I think the normal salt solution is best. The object of the local treatment of diphtheria is not to destroy the bacilli; that is practically impossible; the object is not to remove the pseudo-membrane; that is impossible without doing more injury than good. The Loeffler bacilli may be lodged in the throat for an indefinite period without pro-

ducing any symptoms unless there is an erosion of the mucous membrane. All we can hope to do by means of local treatment is to wash away the septic products. Certain experiments in which we are now engaged tend to show that the local irritability and the toxic symptoms are lessened by the use of very hot salt solutions; for this purpose we have been able to employ a solution at a temperature of 140° F. In cases where there is a great deal of obstruction apparently due to inflammatory swelling, I know of nothing that will afford so much relief as very hot irrigations; the beneficial effects can be almost immediately observed on inspection. At the temperature above mentioned it has been shown experimentally that the Loeffler bacilli are destroyed. In our experiments, the temperature of the water was 140° F. when it left the syringe; it was probably several degrees cooler before it reached the throat.

As regards the use of a proper syringe: When irrigation, as a method of treatment in diphtheria, was first described by me in this Academy four years ago, great stress was laid on the use of the Davidson syringe, in preference to the fountain syringe, because I believe that in the treatment of nasal diphtheria, the latter is absolutely useless.

With reference to the use of steam: Four years ago I expressed my belief that the croup tent, such as has been described to-night, is a dangerous element in the treatment of diphtheria. To shut up a child in a tent which is constantly filled with steam, and is practically equal to a Turkish bath, is a most pernicious measure. Better not use steam at all than to use it in that way. I think the proper way to use it is to attach a rubber tube to the nozzle of a kettle filled with boiling water, and then carry the end of the tube as near to the child's mouth as possible, but always keep it three or four inches below the level of the mouth. In this way the patient continually breathes the heated air, but the entire body is not immersed in it. This method may be kept up indefinitely without exhausting the patient.

When bichloride was first recommended as an internal remedy in the treatment of diphtheria, it was supposed to act as a germicide. We now know that the germs do not enter the circulation, and that the constitutional symptoms are due to the action of the toxins, upon which the bi-

chloride taken internally can have no possible effect. In toxæmia it seems to me that bichloride is contraindicated.

With reference to calomel sublimations: I do not think they have any specific effects on the germs; they certainly have no germicidal effect. I believe that these sublimations do good simply by stimulating the secretions, and promoting the separation of the false membrane. At the same time, the drug is poisoning the patient. This I have seen repeatedly. Just as good effects, I think, can be obtained from the inhalation of sulphurous acid gas.

DR. J. W. BRANNAN.—Certain observations recently made by Roux of Paris, go to show that the bacteriological test in cases of diphtheria may indicate whether the attack is likely to prove mild or severe. It is well known that in diphtheria we often have a mixed infection, and as the infection is shown to be mixed or pure, so the prognosis differs accordingly. I think Roux states that in pure diphtheritic angina, the mortality ranges from 5 to 10 per cent., but when we find streptococci or staphylococci also present in large numbers—more especially the former—the mortality rises as high as 75 or 80 per cent. or even higher. At the Willard Parker Hospital we have taken steps to test the accuracy of these observations, and thus far I think the results have entirely verified the statements of Roux.

Dr. Berg, in his paper, referred to the high mortality attending the disease in the hospitals abroad. One reason for that is that those hospitals are devoted exclusively to children; if we excluded adults from our hospital, the mortality rate would probably be very nearly the same as that reported by writers abroad.

DR. A. CAILLE.—A proper estimate of the virtues of antitoxin will depend on the kind of cases in which we employ it. The mortality of the disease in Europe ranged from 40 to 60 per cent., but since the introduction of antitoxin, the mortality has been reduced to 15 to 25 per cent. If antitoxin will reduce the mortality in severe, undoubted cases of diphtheria, then we can safely say that we have accomplished a therapeutic achievement of the first magnitude, one of which we may justly be proud.

Thus far I have employed antitoxin—both Behring's and Aronson's—in sixteen cases; of these, two died from

heart failure. This tallies very closely with the reports from the other side. What may we expect from antitoxin, and what may we not expect? We may expect from it a specific action on the Klebs-Loeffler bacilli and arrest of the diphtheritic process, and furthermore, we may expect from it an immunifying power. We should, however, not expect too much. If it is not used early, if it is used after the system has become thoroughly invaded by the toxins, after the heart is crippled and after the onset of renal complications, it would be absurd to expect recovery. Furthermore, even in cases where the antitoxin is employed, stimulation should not be omitted, and mild local treatment is absolutely necessary.

In conclusion, I wish to add just one word of warning regarding the genuineness of the preparation we employ. None should be employed excepting from an absolutely reliable source. All others should be regarded with suspicion, for they may not alone not contain the antitoxins of diphtheria, but even contain the toxins of the disease and thereby do great harm.

Dr. C. A. LEALE.—Experience has taught me that it is very difficult and sometimes extremely dangerous to attempt to sufficiently feed a child having pharyngeal diphtheria, while the child is held on its back, as recommended and illustrated by the picture shown on the screen this evening. To maintain life in this disease we have to give a very large amount of nourishment, which must either be liquid or pulpous. To give liquids by the dropper would require too much of the time necessary for the required rest of the nervous system and in diphtheria of the throat we have more or less paralysis of the muscles of deglutition, which often causes liquids thus fed to an infant to pass into the larynx and produce spasm of the glottis, which is one of the most distressing symptoms occurring in the treatment of this dreadful disease, a condition familiar to all physicians who have battled with this great enemy of infantile life. The appeal of the anxious parents, who sympathize with the patient, to let the little one die in peace, may produce an impression to desist, even when the attendants know that a very large proportion of even the bad cases of diphtheria can be saved by the necessary fresh air, proper food and medication.

For many years, to complete the necessary nasal and

posterior pharyngeal douching to cleanse away the accumulated obstructing desquamative exudate of diphtheria, I had used a syringe and a soft India-rubber No. 8 or No. 10 velvet-eyed catheter, passed through the nostril, first washing one side, causing the flow from the opposite nostril, then deeper, permitting the effluent to run from the mouth as the head was held slightly forward; and once while attempting, I found that the catheter had passed in further than usual and into the œsophagus and that all the liquid used for the douching had been sent into the stomach, where it was retained as a refreshing drink.

Before this for years I had fed children in the semi-unconscious state, or with throat troubles, through the nostrils, while lying on their backs, by means of a teaspoonful of liquid at a time poured through the cleansed nostrils, which readily passed into the stomach; but after discovering how easily children could be fed by means of the nasal tube, I have resorted to this method and for more than ten years have instructed our resident physicians in our children's hospitals how easily it can be done. I had found that dropping liquid into the throat would very often produce strangulation, that nasal feeding of liquids by the spoon less often, but by the tube properly introduced through the œsophagus hardly ever.

The best method of easily and safely feeding children having diphtheria, even while the O'Dwyer tube may be in the throat, or while the tracheotomy tube is in the place made by the external surgical opening; also for feeding infants in the unconscious state following convulsions or in the coma from brain trouble is as follows: Let the nurse sit on a solid chair and hold the child in the sitting posture, erect on her lap; place the right arm of the child well under the left arm of the nurse, while the nurse with her left hand firmly holds the head of the child closely up against her left axilla, and with her left elbow restrains the left arm of the child. The nurse by separating her knees and letting the legs of the child drop between them can thereby secure quietness of the lower extremities of the child. The right hand of the nurse can now easily control all the other parts of the child in a fixed position and to the great security and comfort of the child.

Everything being in readiness, and all necessary articles on a table within the reach of the physician, begin by first

moistening with gum arabic mucilage a soft, pliable No. 10 India-rubber velvet-eyed catheter, which is held up by the thumb and index finger of the left hand of the physician, allowing the proximal end of the catheter by hanging directly downward to reach the tip of the nose of the infant; watch for the end of the act of expiration, and, during the inspiratory act, steadily but quickly by means of the thumb and index finger of the right hand push straight backward until from seven to nine inches have been inserted through the nose; the first pinch of the catheter should be about one inch from the point of the catheter, and after this has been pushed in, another inch, pinched and pushed quickly in inch by inch, until the lower end of the œsophagus or stomach has been reached. If pushed too far into the stomach, the irritation may cause temporary efforts at vomiting, which may be prevented by withdrawing the catheter for a short distance, but if persistant irritation follows, which very rarely occurs, then instantly draw out the catheter entirely, and after a rest try again.

The tube now being in place, calmly hold it there, thereby assuring the child that it is safe and can breathe. Now by means of a syringe inject from six to twelve ounces of strained liquid food, and, at the end force down a little air to thoroughly empty the tube. Drinks of water, barley gruel, or soup may thus be given, after which, still having continuously held the tube by the left hand, now seize the distal end with the thumb and index finger of the right hand and, during an expiration, steadily withdraw, never for an instant stopping until the tube has been thrown away out of sight of the child. This mode of feeding is so easy, safe, and effectual, that in less than one minute of time the entire procedure may be accomplished.

DR. L. FISCHER.—Up to the present time, I have treated 112 cases of diphtheria with antitoxin, and of these four have died. Even this favorable mortality rate could have been lowered if the cases had been more carefully chosen. I do not see how we can hope for any good results from antitoxin in cases that are practically moribund when the treatment is commenced. Furthermore, it should only be used in cases of pure diphtheria; cases in which there is a mixed infection, as stated by Dr. Brannan, usually result fatally. It should not be employed in cases where the

diphtheritic angina is complicated by another disease, such as scarlet fever or measles. The dose of antitoxin is now considerably larger than was at first advised. In some cases as much as five c. c. were injected at once.

DR. BERG (closing the discussion).—I have nothing to say in closing, excepting that in a subject of such magnitude as this it is very natural that there should be considerable difference of opinion expressed.

Stated Meeting, December 19th, 1894.

THE INFLUENCE OF THE BICYCLE IN HEALTH AND IN DISEASE.

By GRÆME M. HAMMOND, M. D.

About two years ago I called attention to the treatment of certain diseases of the nervous system by the proper use of the bicycle.[1] This was the first attempt, at least in this country, to call the attention of the medical profession to a therapeutic agent undoubtedly of great value. At that time comparatively few physicians had had any practical experience with the bicycle, but since then wheeling has taken hold of the popular fancy to such an extent that thousands now ride where formerly hundreds rode, and the medical man who at one time considered bicycle riding degrading to his professional dignity, now joins with his lay brothers in the pursuit of health and pleasure on the wheel. The universal popularity of the wheel—and it seems to be growing in favor every year—has undoubtedly not only been a source of great pleasure to thousands, but must also have had an appreciable effect upon the health, either beneficially or the reverse, and physicians are now arriving at a more intelligent conception of the possibilities of the wheel, both for good and for evil. The medical journals of this and foreign countries are daily adding to the literature of the subject by the reports of diseases which seem to have been induced, or are aggravated, by the use of the wheel.

The question concerning the deleterious effect on the human organism by the injudicious use of the wheel concerns the numerous specialists in medicine as well as the general practitioner. There is hardly an organ in the

[1] Journal of Nervous and Mental Diseases, January, 1892.

body which may not be influenced one way or the other by the proper or improper use of the bicycle; and hence it is that all medical men should study this question carefully and endeavor to arrive at the exact facts of the case. To this purpose I propose to consider the subject from the following standpoints: 1. The use of the cycle by persons in health; 2. The use of the cycle by persons diseased.

Little need be said about the rational use of the bicycle by healthy persons. It is only when the wheel is improperly or immoderately used that it constitutes a source of danger. The question will at once arise as to what constitutes this moderate and rational use. As men differ, among other things, in points of age, temperament, physical strength, and power of endurance, so is it absolutely impossible to prescribe any general rule which will be applicable to all men. Each individual must formulate rules for his own guidance, and if he is incapable of doing this, either because he has not arrived at years of discretion, or because he recognizes his ignorance and distrusts his judgment, his medical adviser should be consulted, who by studying his physique, heart measurement, and lung capacity, can judge with considerable accuracy the amount of exercise which is most conducive to healthy physical development. But, as a general rule, the average individual, after a fair amount of experience, becomes quite competent to determine his own capabilities and requirements.

Let it be granted that an individual uses the wheel properly, what results should he reasonably expect to achieve? Statistics do not supply any information on this subject. No attempt has as yet been made, that I am aware of, to study the physical development of men who have been using the bicycle properly for years. I say for years, because the best effects of any form of exercise which is practised for the purpose of obtaining physical development, are never obtained rapidly, and, particularly in young subjects, should only be studied after several

years of continuous culture. There being no available literature on the subject, I am obliged to depend solely upon my own observations. This source of information must, necessarily, be very meagre. Although there are thousands who ride, there are comparatively few within my reach who have been riding long enough to render statistics on their physical development of much value. I have, however, collected fourteen cases of amateur riders who have kept accurate records of the years they have been riding, and approximate records of the number of miles they have ridden in this time. These cases I have examined, and endeavored to tabulate the most important features ascertained. These men have ridden from five to thirteen years, and in that time have traveled from five thousand miles to twenty-seven thousand miles.

TABLE No. 1.—AMATEUR RIDERS.

Cases.	Age.	Ridden a Wheel, Years.	Miles Ridden.	Chest expansion, Inches.	Heart.	Muscular System.
1	37	5	22,000	1¾	Slight hypertrophy.	Well developed.
2	25	10	27,000	1¼	" "	Legs well developed, arms and trunk fair.
3	36	5	8,000	2	" "	General development greatly beyond normal.
4	42	12	24,000	1½	" "	Well developed.
5	46	13	25,000	1	Normal.	Average.
6	24	13	14,000	1⅞	Slight hypertrophy.	Well developed.
7	25	10	20,000	1¾	" "	" "
8	34	7	18,000	1¾	" "	" "
9	30	8	9,000	1⅜	Normal.	" "
10	29	7	8,000	1⅞	Slight hypertrophy.	" "
11	26	7	17,000	2	" "	Generally very muscular.
12	24	6	7,000	1¼	Normal.	Average.
13	39	5	6,000	1⅛	"	"
14	24	5	5,000	1½	Slight hypertrophy.	Well developed.

Average chest expansion, 1 4-7 inch. Heart usually hypertrophied without dilatation. Average age, 31 4-7 years. Chest expansion, 13 riders above normal, 1 normal.

This table demonstrates two important facts very clearly. First, that men who have ridden a great deal for a number of years have acquired simple cardiac hypertrophy without dilatation; and second, that their breathing capacity is greatly in excess of that of the average man. The cardiac hypertrophy that I refer to is due to a simple increase of muscular tissue, and is in the nature of a healthy growth or development induced by exercise. It is precisely similar to the hypertrophy which occurs in any other muscle which has been used a great deal. Hypertrophy in this sense is purely a relative term. For more than a century man has shown a tendency to develop the different qualities of the mind, and has paid but little attention to the gradual degeneration of the muscular system and to the consequential reduction of his powers of physical endurance. This has been one of the natural consequences of the rapid advance of civilization, especially in this country, and this, together with the great development of industries, commerce, and the professions, which have demanded the best energies of our men, finally made us a race of intelligent, clear-sighted, energetic, and nervous people, but very far short of the physical perfection seen in men in earlier ages. The dangers attending this physical degeneration of the race have of late years been gradually instilled into the public mind, and this has resulted in a healthful reaction. Men and, what is of almost equal importance, women are gradually beginning to comprehend the advantages of physical development, and are in a fair way to understand that the highest type of man, taken as a whole, is not represented by intellectuality alone, but more properly by a judicious combination of mental and physical vigor. Ten or fifteen years ago it was exceptional to find business and professional men and women who practised any special form of exercise. To-day thousands are making physical exercise an important feature of their daily duties; and what is of greater impor-

tance, we are educating our children to believe in physical culture and to practise it. The evolution of the bicycle to the form it presents to-day is probably the greatest factor which has influenced the spread of the doctrine of physical culture in this century. As a natural consequence of this gradual but universal physical development, which will only be attained in time, we must expect to observe a relative development of some of the internal organs, particularly of the heart and lungs. The simple hypertrophy of the heart observed in most bicycle riders who have ridden to any extent, will gradually become more common, and finally will be accepted as normal, and what is known to-day as the normal heart will then be considered degenerated.

A second glance at the table shows that all the riders have a greater breathing capacity than the average man. The chest of the average man expands an inch when the lungs are inflated to their greatest capacity. This has been carefully verified by myself and by others, who have measured many men whose lung powers have not been developed by special forms of exercise. It is usually believed that the average chest expansion is three inches, and there are many who can increase the girth of the chest much more than this. It is claimed, for instance, that Sandow's expansion is thirteen inches. This, however, is fallacious and does not by any means express the truth. It is accomplished by a forced expiration before the inspiration, and is then further augmented by "setting" the scapular and pectoral muscles. In the table I have prepared, the average lung inflation is represented by a chest expansion of one and four sevenths of an inch, which is an excess of four sevenths of an inch above that of the average man.

This excess is unquestionably of great advantage to the individual possessing it, when it is taken into consideration what a great influence is exerted on the general

health and on the proper performance of the functions of the various organs by a perfect oxygenization of the blood; and when there is added to this the action of a powerful, muscular heart, it can readily be perceived that bicycle riding, conducted properly and for a long time, induces a condition of cardiac and pulmonary development which must exert an enormous influence in maintaining the proper functions of other organs; in preparing the organism to resist disease; and in overcoming disease that has been contracted.

The muscular system of the bicycle rider is, as a rule, uniformly well developed. It is often claimed by those who are not properly informed, that the exercise develops the muscles of the legs at the expense of the rest of the body. This is not so. On the contrary, the muscles of the back, chest, arms, and abdomen are employed a great deal—not to the same extent as the muscles of the legs, but sufficiently so to consider bicycle riding an exercise inducing general development of the muscular system.

The observations recorded in Table No. 1 were made on men who have always ridden in the erect posture. A great deal has been said in regard to the pernicious effects of riding with the body almost, if not quite, at a right angle with the legs. A peculiar malformation of the spine has been reported to result from this posture, and it certainly seems as if the lungs, and consequently the heart, must be greatly hampered in performing their work. To obtain some information on this subject I have carefully examined fourteen professional riders, seven of whom are the most celebrated racing men in the world—men who have obtained national reputations as great riders; who have ridden more races than any other men; and who have ridden faster than any other men; and who always ride with the body at right angles to the legs. These observations I have tabulated in Table No. 2. In no case was there any deformity of the spinal column, and

I may say here, if any such condition has ever been observed, the number of cases of it must be very few indeed. I have never seen a case of it. They were all well-built, muscular men, but differed from those in Table No. 1, who were all amateurs, in that in all cases there was an excessive development, compared to the rest of the body, of the muscles of the thighs, particularly of the *vastus internus* and of the muscles of the abdomen.

TABLE No. 2.—PROFESSIONAL RIDERS.

Names.	Age.	Remarks.	Chest Expansion. Inches.	Heart.	Muscular system.
Zimmermann	24	American world's champion.	1¼	Hypertrophied.	Well developed. Extraordinary development of abdominal and thigh muscles.
Wheeler....	25	American. Celebrated rider.	1⅞	"	Well developed. Extraordinary development of abdominal and thigh muscles.
Edwards....	23	England's champion.	1⅞	"	Well developed. Extraordinary development of abdominal and thigh muscles.
Verheyn....	20	Germany's champion.	1½	Slight hypertrophy.	Well developed. Extraordinary development of abdominal and thigh muscles.
Colombo....	23	Italy's champion.	2	Slight hypertrophy.	Very muscular. No special development.
Lesna.....	31	France's 24 hour champion.	2	Slight hypertrophy.	Very muscular. No special development.
Martin.....	25	America's 6 days champion.	2	Hypertrophied.	Very muscular, especially in the thighs.
Albert.....	28	American. 6 days rider and short distance racer.	1½	"	Too slender for his height. Muscles not at all prominent except in abdomen.

TABLE No. 2.—PROFESSIONAL RIDERS.—*Continued.*

Names.	Age.	Remarks.	Chest Expansion. Inches.	Heart.	Muscular system.
Berlo.....	28	American. Short distance racer.	⅞	Hypertrophied, but not strong.	Muscular thighs and abdomen. Chest somewhat sunken.
Knowles....	40	American 6,240 miles in 60 days racer.	¾	Hypertrophied.	Very muscular.
Foster.....	40	German. Long distance rider.	¾	"	Normal development.
Gross	28	American. Short distance racer.	1	"	Well developed.
Murphy, W. F.....	25	American. Class B racer.	⅞	Hypertrophied, but not strong.	Not at all muscular except in thighs and abdomen.
Macdonald..	18	American. Class B racer.	1¼	Normal and very strong.	Small, but well developed.

Average age, 27 years. Average chest expansion, 1 3-7 inches. Chest expansion, 9 above normal (these are celebrated riders); 1 normal (1 inch); 4 below normal.

The development of the abdominal muscles resulted from their continual contraction while maintaining the riders in their racing position. All had cardiac hypertrophy without dilatation. At first glance it would seem as if the lung capacity of the professionals was fully as good as that of the amateurs, and this is true if the measurements of the national champions only are considered.

Table No. 2 gives the measurements of fourteen celebrated professional riders. Table No. 1 gives the same measurements in an equal number of amateurs. Comparing the measurements of amateur riders with those of the professionals, who are national champions, very little difference can be observed. These men who ride at racing speed, doubled up like a jack-knife, were as well developed, had just as powerful and healthy hearts, and their lungs could consume as much oxygen as those who sat upright and rode at moderate speed. But it must

not be concluded from this that I mean to infer that *any* man can race and ride in the posture of a racer with impunity. Far from it. These men became professionals because they found that nature had particularly adapted them for fast riding. They make a business of it, and are always in the best possible physical condition.

If we turn, however, to the measurements of the other six professionals whose inferior capabilities have not allowed them to become national celebrities, we find a very different state of affairs. One has the normal chest expansion of one inch, four below normal, and one is above normal. The latter, however, has been racing but a short time, having recently graduated from the amateur ranks. Comparing the fourteen amateur riders with the fourteen professionals, it will be seen that the average chest expansion of the amateurs is 1 4-7 of an inch, while that of the professionals is 1 3-7 of an inch. The expansion of thirteen amateurs is above normal, and in one it is normal. With the professionals the expansion is above normal in eight, normal in one, and below normal in four. It is quite probable, if professional riders of less phenomenal abilities were substituted in the second table in place of the national champions, the average chest expansion would be found to be below normal.

Excessive work on the wheel is, for the average man, unquestionably injurious, and it seems a fair inference to draw, from a study of these cases, that while many men will, in the course of time, ascertain that nature has adapted them so they may race with impunity, for a time at least, others who are led to emulate their example will certainly injure their health and undermine their constitution. This is particularly true of young lads, many of whom consider themselves the coming racing champions of the world. They can be seen in the streets and on the country roads, tearing along at the top of their speed, the body thrown forward, the back humped and the arms

fixed and rigid, the chest necessarily contracted and preventing its proper expansion, at the very time when free movement of the chest walls is most necessary. In such cases healthy development is likely to be arrested, and cardiac dilatation, impoverishment of the blood, and lowered vitality of the system are probable consequences. Parents and physicians should use their authority to prevent this abuse in the rising generation, and a youth who will not ride properly should not be allowed to ride at all. It has been claimed that appendicitis can be sometimes attributed to this faulty position in riding. While it is undoubtedly true that persons who ride the wheel, no matter in what posture they ride, may have appendicitis, there is nothing in literature to prove any direct relation between appendicitis and bicycle riding as a cause and effect.

Riding great distances at rapid speed is an abuse of the wheel which cannot be too severely condemned. Any one who witnessed the last six days' bicycle race about a year ago, canuot but recall with disgust the sickening exhibition of exhausted riders, having to be lifted from and onto their wheels and carried to and from their quarters, their joints swollen and inflamed, barely able to see, and only kept in a conscious condition by the united efforts of trainers and physicians. This, however, is an extreme case, but it is not at all uncommon for large parties of riders of both sexes to go on what are called "century runs," that is, to ride a hundred miles often within a limited number of hours. To many riders of experience, who are in good condition and possess excellent powers of endurance, this may not be a difficult feat to perform, but many others in making the attempt suffer from exhaustion which leaves its traces for several days. There are others again, who travel shorter distances, but who ride at such speed that they return home thoroughly tired out. This, if it occurs only occasionally, is perhaps of little moment, but when the repetition is frequent it is undoubtedly injurious.

Moderate exercise, properly performed, invigorates the system, strengthens the muscles, and increases the powers of physical endurance, but excessive and exhaustive exercise does none of these things; on the contrary, it is decidedly injurious to health and should never be encouraged. Under certain conditions, diseases of the genito-urinary tract may with great probability either be induced, or else, if they already exist, they may be aggravated. Cases of prostatitis, urethritis, and cystitis have been reported, which seem to show quite conclusively that the friction or jolting of the perineum against the saddle was the exciting or aggravating cause. Such cases are due to the carelessness or ignorance of the rider. An individual in riding the cycle, just as in riding the horse, should sit upon the gluteal muscles and not upon the perineum. All bicycle saddles can be tilted either forward or backward, and every person who rides should so adjust the saddle that the weight of the body is borne by the gluteal muscles and not by the perineum, otherwise it is quite probable that injurious results may follow.

In regard to the excitation of the genital organs in both sexes, I am quite confident that this never occurs, unless it is done purposely or else by the greatest negligence. A woman who wishes to obtain friction of her genitals on the wheel can easily do so by adjusting her saddle for that purpose. But if she does so, her position will be so uncomfortable as to deprive riding of all enjoyment. She may ride for the purpose of obtaining genital irritation, or she may ride with perfectly proper motives, but she cannot do both at the same time.

In the male, the case is different. Riding upon the perineum may in many instances cause genital irritation, but this can be effectually prevented by the proper adjustment of the saddle. It can therefore be seen that these diseases or conditions are by no means necessary consequences of bicycle riding. On the contrary, they can be

remedied in all cases by the intelligent comprehension of the wrong and its remedy.

The question is often asked, Shall women suffering from abnormal conditions of the uterus and ovaries ride a wheel? I was much pleased, at a recent meeting of the Gynecological Section of the Academy, to listen to the opinions of our leading gynecologists on the subject. It seemed to be the general opinion that this form of exercise was not only not injurious, but that in many cases of disease it was highly beneficial, either as a means of relieving local congestion, or else acting indirectly on the pelvic organs by improving the general health of the patient.

There is one other condition which deserves mention, and which I am not aware has called forth any comment up to the present time, and that is the effect of bicycle riding on the air-passages and as a cause of mouth-breathing. On examination, I found all of the men enumerated in table No. 2, who were professional riders, breathed through the mouth while riding, and that eight of these were habitual mouth-breathers. The majority of the amateur riders I have questioned on this subject admit that they breathe through the mouth while riding, but whether the proportion of mouth-breathers is greater among cycle riders than among those who do not ride, I do not know, but it is a matter that deserves investigation.

The use of the wheel by persons diseased, when considered in the light of a therapeutic agent, is a subject of considerable importance. It also leads us to inquire whether there are not physical conditions under which the bicycle should be absolutely prohibited. In certain diseased conditions of the heart, bicycle riding may prove very beneficial. In simple degenerated conditions of the muscular fibres, in dilated hearts either with or without compensatory hypertrophy, and in slight valvular affections, bicycle riding, when properly practised, may prove

of great service, because it improves the nutrition of the organ and develops the muscular fibres, thereby enabling the heart to perform its work more effectually. But no one suffering from any cardiac affection should ever ride a wheel, unless advised to do so by his physician; and in such instances the physician should make frequent examinations in order to determine whether benefit is being derived from it or not. Where exercise is advisable in heart affections, I know of no better method of obtaining it than by the proper use of the wheel. By riding slowly and on an approximately flat surface, the mildest cardiac exercise can be obtained, and this, as the heart improves in strength, can be increased by degrees, and in direct ratio with the cardiac development. By this means the strength of the heart can be greatly increased, thus causing a natural compensation for many abnormal conditions.

But bicycle riding can cause as well as cure cardiac degenerations. If an individual compels his heart to work beyond its capacity and the abuse becomes in any sense continuous, the danger of cardiac degeneration becomes imminent. This is more particularly liable to occur in subjects whose cardiac muscular fibres are abnormally weakened from disease, dissipation, or from long neglect of such exercises as are requisite for maintaining the heart in a healthy condition. In valvular lesions, also, immoderate riding will unquestionably aggravate existing abnormal conditions, and perhaps superimpose another cardiac disease upon the original one. In short, it may be said that bicycle riding may benefit or injure the heart according to the condition of the heart and the manner in which the individual conducts his exercise, and the general principle may be adduced that, while the moderate use of the wheel is conducive to cardiac development, the excessive use endangers cardiac degeneration. I cannot agree with the decision of the French Academy,

that "no one should ride a wheel without consulting a physician;" but this rule certainly should apply to all persons who have cardiac disease and to all others whose general health, vitality, and physical strength are to any degree below the normal. What has just been said in relation to cardiac diseases applies in the main to pulmonary affections. I have already shown, as far as the limited number of cases at my disposal permits, that bicycle riding, when practised by a healthy person, unquestionably develops the lungs and increases their capacity greatly beyond what is now considered to be normal. The knowledge of this fact may be taken advantage of in the treatment of those pulmonary conditions in which a breaking down of tissue will be the natural and probable consequence in the course of time. Moderate exercise of the lungs by the moderate use of the wheel practised systematically and for a long time, will undoubtedly retard and perhaps completely arrest some degenerative changes in the lungs. This will follow not only from the direct effect of pulmonary development, but also from the generally improved physical condition which is always a prominent feature of bicycle riding when rationally practised. On the other hand, I can conceive of nothing which will be more injurious to certain abnormal pulmonary conditions than the excessive use of the wheel. Not only will this tend to accelerate degenerative processes, but it may also induce them in persons predisposed, particularly if the general vitality is simultaneously diminished.

The same general rule can be formulated here that seemed to be applicable to the heart, that is, that the moderate and proper use of the wheel results in pulmonary development, while the excessive use may be followed by injurious consequences.

In my former paper I showed, by the report of a number of cases, that in certain diseases of the nervous system

bicycle riding was very serviceable. That report referred mainly to cases of functional and organic paralysis, and to neurasthenia and hysteria, and demonstrated that in these conditions great improvement was obtained directly from the use of the wheel. My opinions were so fully expressed at that time that it is needless to refer to the subject again, except to state that my experience in the past two years, since that paper was published, fully corroborates the views I then entertained. I only desire to state in addition that I have carefully studied the effects of bicycle riding on numerous cases of functional dyspepsia and constipation, and have observed great benefit in most instances, but only in those cases in which the riding was followed systematically. There are two other conditions I desire to refer to before I conclude what I have to say, in which bicycle riding seems to exert a beneficial influence. These are gout and diabetes. Possibly both of these diseases are benefited by the great quantity of oxygen consumed; by the improvement in digestion and assimilation; and by the effect that prolonged muscular action has on eliminating certain substances from the system which are either poisonous *per se*, or else become so by conversion. In two cases of subacute gout, I have prescribed the use of the wheel and have every reason to feel satisfied with the results. In only two cases of diabetes have I had the opportunity of making observations in regard to the effect of this exercise on the excretion of sugar. These few cases are not sufficient to allow the presentation of any absolute deductions, but the results obtained were sufficiently encouraging to warrant further investigations in this direction. If we accept the view that the manifestations of gout depend upon an excessive formation of uric acid in the system, it can be readily understood, theoretically at least, how bicycle riding can modify or arrest this process. First, by the improvement in digestion and assimilation; second, by the greater demand upon

the nitrogenous compounds required by the repair of the muscles; third, by the increased oxygenation of the blood, which increases the elimination of urates; and fourth, by the further elimination of morbid material through the increased activity of the sweat-glands.

This theory may be susceptible to criticism, but whether it is correct or whether we must look further for the reasons, the fact remains that benefit is derived in these cases from the use of the wheel. In the two cases of diabetes, experimental studies showed that in both cases, and with no change of diet, the amount of sugar excreted was diminished when the bicycle was used regularly. The explanation of this fact seems to be reasonable, and is similar to that just offered in reference to gout. The waste taking place in the muscles is repaired from the nitrogenous elements, which otherwise would be converted into urea and then into sugar. The larger the muscles are that are wasted, the greater quantity of nitrogenous substance will be required to complete repair. The largest muscles in the body are in the lower extremities, and consequently bicycle riding, which calls these muscles into active use better than any other form of exercise, operates to excellent advantage. The heart also need not be exercised unduly, which, in these cases, is often a point of considerable importance. It is impossible, in an article on this subject, to enter deeply into the discussion of each one of the subjects touched upon without making a paper of interminable length. I will therefore conclude what I have to say by the statement, that experience has shown me that bicycle riding for a healthy individual is one of the most excellent forms of exercise for maintaining health, retarding disease, and strengthening the constitution, and is also a pleasant recreation for the mind; and that in many forms of disease, when used cautiously and under medical supervision, it will often be found of inestimable advantage.

DISCUSSION.

DR. A. D. ROCKWELL.—My experience in cycling during the past five years has been somewhat extensive, and I only regret that I did not begin to ride five years earlier. Further than this, I have under my observation a score or more of patients who indulge in this form of exercise. While I am an ardent advocate of cycling, still I do not advocate it without restrictions, for I believe that more evil comes from it, as ordinarily attempted, than from other common forms of exercise. We must discriminate between the evil and the good resulting from it. I think that one of the most serious evils attending it is overstraining. When a man confines himself to walking, there is little danger that he will overstrain himself. With cycling, however, it is different: the rider, perhaps, goes far out in the country, where new sights continually greet his eye, and in this way one takes so little heed of the amount of ground covered that the strength is very easily overtaxed. Such a ride, instead of being invigorating, produces profound languor. I have in mind a young man who illustrates this point. For two years cycling was his sole method of exercising, and I have known him to cover one hundred miles in one day. He finally developed an irregular and intermittent pulse, which become normal again when he began to ride more judiciously. I am almost ashamed to confess that in my own person—in my earlier days of cycling—a similar and very disagreeable temporary condition was produced by my attempts while off on a tour to keep pace with those who were younger and possessed more endurance.

I think perhaps it would be well for every one who contemplates riding a bicycle to first ascertain the condition of his heart, especially those who have reached middle life or over. I would suggest the following simple test to indicate the limit of physical strength: when one can no longer breathe easily and with entire comfort through the nose, with the mouth closed, it is time to stop. There are perhaps other evils connected with cycling, but I think they are over-balanced by the good. I agree with Dr. Hammond that a weak and nervous heart is not necessarily a contra-indication to this form of exercise. On the

contrary, I believe that patients with heart trouble may often be benefited by it. Speaking in a general way, I think it is the ideal form of exercise for lithæmic patients, and those suffering from dyspepsia or torpid liver. If every man would live temperately, taking only just a sufficient amount of food to supply the waste of the body, we might get along with a minimum amount of exercise, but this is not so. The table offers temptations which we cannot resist, and in my experience cycling affords the most valuable and efficient as well as the pleasantest means of enabling the body to get rid of the surplus material.

DR. S. D. POWELL.—Although I am only a beginner on the wheel, I am a firm believer in that form of exercise. I commenced to ride last May, and since then I have reduced my weight twenty-six pounds and reduced my age twenty-six years—for I feel that I am fully that much younger. I regret, more than I can express, the opportunity which I lost quite a number of years ago, when a friend of mine offered to allow me to ride his wheel. I refused his offer then, because it carried my thoughts back to 1868, when I first entered the University of Virginia; coming from a Southern country, and having never before seen ice, I was seduced in a weak moment to try on a pair of skates, being assured that if I struck out boldly and independently, no one could tell whether I had ever skated before or not. I acted on that suggestion, and that was the reason why I declined my friend's offer to ride his wheel. I was afraid of it. Last May I was induced to try the wheel because while indulging in a little parental authority with my son regarding the subject of riding, he informed me that I "didn't know anything about it." Since that time I have ridden often, and enjoy it exceedingly. The popularity of the bicycle, in my opinion, is due to the fact that it furnishes the greatest amount of physical development combined with the greatest amount of pleasure, and at the least cost. I mean at the least pecuniary cost and also at the least cost to the general health. I am speaking of the use of the wheel in health, not in disease. If a man adopts any form of exercise, it does not matter whether it is riding a horse or a wheel, or playing football, or walking or running, if he stays well within the

limit, he must of necessity be benefited by it; if he exceeds that limit, then he must pay for the result. Every man's limit of endurance is different, and in my opinion it is useless to speak of the ills which come from cycling without taking into consideration the fitness of the man for the amount of exercise which he indulges in. A healthy man, woman, or child can ride a wheel in moderation just as safely as he can walk or run in moderation. None of us, if we are well-balanced, are apt to overexert ourselves physically.

There is nothing, in my judgment, which offers greater inducements of pleasure and benefit to the professional man than the moderate use of the wheel after his day's work. That the muscular development induced by it is not confined to the legs alone is very easily demonstrated, for by overdoing it you will find that you ache from head to foot; there will not be a pin's point about you that doesn't ache. I do not think that a few hours' ride on the wheel daily, no matter what position the rider assumes, will produce any deformity of the spinal column. I have never seen such a result from it. With reference to its effect on the prostate, my opinion is that if a man has a diseased prostate, sitting on the wheel is likely to irritate it and increase the damage, and this is also true as regards horseback riding; if there is no prostatic disease, then riding the wheel is not likely to produce it. The wheel must not be held responsible for the defective development of the brains of certain people who adopt that form of exercise. There are some cyclists who have not the least idea as to what they expect to gain by this method of exercise, whether pleasure, or health, or the development of a certain set of muscles, or to overcome a certain disease; they simply adopt it as a fad, without rhyme or reason, and run it to the ground. Fools live and will continue to live so long as the world exists, and it is manifestly unfair that a healthy, life-giving exercise should be held responsible for the bad judgment of athletes or would-be athletes, and people of defective intelligence.

DR. W. M. POLK.—I fear, Mr. President, I am called upon because of your politeness rather than in consequence of any definite knowledge on my part of this subject, or perhaps it is because of the fact that some time

ago we both contemplated taking this form of exercise together. While you, I believe, are an expert rider, I am but an amateur of the amateurs. I was beguiled into it by the suggestion of some one that long life and perhaps joy was to be found in the use of the bicycle. The long life, of course, is yet to be proved; the joy thus far has not been an unmixed one, for in my endeavors to master this mechanical mustang I have been forced to realize that there is a certain amount of danger from the viciousness of the animal. I do not wish to dwell on this, however, and shall pass on to the subject of prescribing this method of exercise for others. In certain ailments belonging to the female sex, I consider cycling beneficial, bearing in mind, however, the fact that to a certain extent each one must judge for herself how far it can be carried on with benefit. We often have very troublesome cases to deal with, which we do not know how to cure, and which we call dysmenorrhœa. I shall not attempt to classify these cases, for the disease is hydra-headed, but simply leave out of consideration those due to inflammatory conditions of the appendages, and confine myself to those produced by circulatory disturbances in the uterus or appendages. In these latter cases I believe this form of exercise will produce a distinct amelioration of the symptoms, and if it will do but one thing for women, viz.: prove the absurdity of the so-called "rest cure" in the treatment of such pelvic ailments, I am quite willing to subscribe to a monument to the memory of the first man who invented the bicycle.

DR. F. VAN FLEET.—After listening to the paper of the evening and the various speakers on the subject, it seems that there is very little left for me to say. The evolution of the bicycle is very interesting, and it is quite likely that the man who invented the original machine never thought that he gave something to the world that would make his name famous, and furnish a subject for discussion to a scientific society many years afterwards. The "safety" bicycle of to-day is the outcome of many previous inventions and modifications, and in it we have an instrument the effect of which will be very manifest on the intelligence and physical development of the human race. It places in the hands of the common people a means of

exercise and pleasure that would otherwise be denied them, and which tends to develop them both physically and intellectually.

The gentlemen who have preceded me have said little about the time at which it is best to ride. It seems to me that during the summer months the early morning—5 o'clock or earlier—is the time *par excellence*. This is the hour when all Nature is smiling, the birds are singing, and the trees and shrubs are at their best. This is the time when the air is clear and man can expand his chest.

I do not agree with the statement made by Dr. Powell that the use of the bicycle produces a reduction of weight. On the contrary, during the past two years, I have gained about twenty pounds. I believe the benefits derived from it are brought about by increased tissue changes. After a hard ride I perspire freely, and then, on reaching home, I can hardly get enough water to drink. This thorough washing out of the sweat-glands must certainly carry with it a good deal of deleterious material, and thus it acts as a preventive to certain diseases. Rheumatism, gout, and allied affections can, I think, be prevented by regular exercise on the bicycle.

DR. S. S. BURT.—I must disagree with the statement made by the last speaker that the best hour for riding is about 5 o'clock in the morning. I do not recommend early exercise, especially before eating. I am enthusiastic about cycling, not that I think it is the only useful form of exercise, but because it is so entertaining that it induces many to indulge in it who would otherwise not exercise at all. A man can probably get just as much good out of walking or gymnastics or horseback riding, but the two former methods become tiresome and the latter is so expensive that it is beyond the reach of most people. As regards bicycle riding in disease, I think that functional disturbances of the heart are benefited by this form of exercise, indulged in with moderation.

DR. H. H. CURTIS.—Dr. Holbrook Curtis said that he had described a bicyclist's pharyngitis which had been observed in France, due to mouth breathing while riding. The low-handled machines should not be employed by those in pursuit of health and lung expansion. Women

make a mistake in riding with too low a seat which facilitates mounting, but causes backache from mal position.

DR. G. M. HAMMOND (closing the discussion).—The views expressed by the various speakers are so much in unanimity with my own that I have nothing to say in closing, excepting to thank them for the part they have taken in the discussion of my paper.

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THE WESLEY M. CARPENTER LECTURE.*

By JOSEPH D. BRYANT, M.D.

PROLOGUE.

I DESIRE, Mr. President, before beginning, to state something of the reasons that actuated me in the selection of this subject for the evening's consideration. While I know full well that I shall say nothing essentially new to-night, yet it occurred to me that perhaps it is not generally known that a steady increase of malignant disease is taking place in all civilized countries, notwithstanding the opposition of modern methods of treatment. In view of this fact it also seemed to me both proper and urgent that I should emphasize the necessity of greater efforts on the part of all concerned to resist this advance. The increased surveillance that is now being employed to defeat the attacks of the more common forms of disease is yielding rewards highly creditable to the forethought and assiduity of the profession. Therefore, may not increased effort, based on logical methods, do something to lessen the gravity of malignant infliction? It is proper too, at this time, that I express my grateful appreciation to those of my friends who have aided me in this endeavor. I mention especially Dr. Roger S. Tracy, of the Health Department of this city, whose knowledge of vital statistics is equaled only by a few, and exceeded by none. Dr. George B. Stewart gave me untiring and intelligent support. Dr. Erdmann, Dr. Titterington, and Dr. Doty also contributed ably to the efforts

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of the others. It is to be regretted that the time at hand did not permit me to overcome the obstacles that were interposed between the anticipated results and the products of the endeavor. I wish through you, sir, to thank the Council of the Academy for the honor conferred on me, and for the expression of confidence which the selection implied, and finally, I hope that any feeling of disappointment that may come to-night as the result of the selection, will be tempered by mercy and charity for the speaker.

IMPORTANT FACTS RELATIVE TO MALIGNANT DISEASE.

The subject of malignant disease is not a new one. The warp and woof of its texture have been spun again and again. Various figures of thought, shades of opinion, with bright perspective, all tinted with hope, have been woven in the fabric only to fade away, giving place to others of a newer pattern, of fresher tints, better symmetry, and, perhaps, of greater endurance. The spinning still goes on, supported by increased opportunity, encouraged by extended experience, and stimulated by the hope of relieving humanity of one of the most inflexible and cruel tyrants in the list of human afflictions. The encroachments of the common foes of human desire stimulate opposition and multiply the number and force of the expedients best intended to resist invasion. The commendable and selfish efforts of those of great power, of unlimited means, and of extended influence are often made irresistible by the forethought, wisdom, and assiduity exercised for their advancement. Those of the human family who are thus circumstanced achieve great results for good and evil, and the opposition which they encounter, while it stimulates the utmost endeavor, nevertheless leavens the result, and the best is thus secured. Men strive for wealth and power that they may gain those things which wealth and power command: the pleasures and comforts of life; the high station in affairs that begets the confidence, respect, and love of fellow men. Men struggle for health and life for the personal comforts and possibilities that they proffer, and to defer that presence which all dread and yet none escape. Man's battle for wealth, power, and fame is largely an individual one, for which he alone has made the

attainment possible. Still, the influences that surround him, the interests that support him, and the principles that claim him, lead directly to the consummation.

Man's battle for health and life is principally a subsidiary one; and the conflict is hardly more than a skirmish on his own part, until he fears defeat. Then he calls a council of war and abides the decision. Thenceforth he is a passive agent, endowed with a responsive intelligence which invites the fullest consideration of his council—the medical advisers.

Whether or not those who are inherently charged with the custody of man's health exercise the same untiring scrutiny and indefatigable vigilance in his physical interest that he himself puts forth to secure pecuniary, official, or social advancement is a question of grave significance, whose answer should not be estimated except on the basis of recorded facts relating to disease. That an increased and concerted effort on the part of the medical profession directed against the hastening pace of malignant invasion is most urgent, is emphasized by the fact that this form of disease is increasing gradually and surely in nearly all civilized countries.

Whether this increase be an apparent one, due alone to the better means of diagnosis, or whether it be an assured one dependent on the influences of civilization, it is difficult to determine. At all events, it matters but little indeed for our present purposes which of these factors is the dominant one, since, that malignant disease is present and must be fought with the resources now at our command is self-apparent to all. Let us hope that a better knowledge of these circumstances will augment the vigilance of the physician in the same degree that it stimulates the aggressiveness of the surgeon. Already the period of post-operation immunity is increased, dependent, no doubt, on the closer observance of disease manifestations and a prompter and more thorough application of the recognized means of relief.

In war, as in well-studied contests for attainment in every other avenue of human activity, the attacking force considers the necessities that justify the combat; then determines the logical lines of the enemy's advance, and final-

ly estimates the sacrifices of defeat and the costs of the struggle before engaging in the conflict. Permit me now to enlist your attention and support in the battle against malignant disease through the same line of action as that which characterizes human conflicts often of the basest nature.

The necessities that justify the combat relate to the loss of human life from the disease and the consequent sorrow and suffering dependent on the sacrifice. Each year the admonitions to duty are repeated with ever-increasing emphasis by the augmenting death record due to malignancy, as revealed by the vital statistics of this and other countries. Permit me to invite your attention for a moment to the established verdict of these statistics.

One has but to glance at the mortality tables here presented to be convinced that malignant disease exacts each year of the strength, comfort, and peace of a commonwealth a constant and liberal toll; and when it is remembered that this ever-increasing demand is inexorable in the procurement, notwithstanding the fact of the interposition of modernized methods of opposition, then indeed it behooves all, lay and professional alike, to unite in a tireless and vigilant crusade against a common, subtle, merciless foe. In 1889 England lost 18,654 of her people from cancer (No. 1), this number being 3·6 per cent. of the entire death-rate for the year, giving an increase of two per cent. since 1865. A more careful estimate shows that the mortality there of the last ten years exceeds that of the preceding ten by 0·85 per cent., which is surely a decided advance. In 1893 London alone lost 3,412 of her population from cancer, this being a percentage of 3·73 of the total deaths for that year (No. 2). This is the highest death-rate from cancer there since 1889; then it was 3·92 per cent., a difference of two per cent. between this date and 1869. The percentage of the last ten years is 3·54, that of the preceding ten years 2·67, giving an increase in favor of the latter period of 0·87 per cent. It is interesting to note the fact that the death-rate from cancer of England and Wales, exclusive of London, in 1892 was 3·62 per cent. of the total death-rate or 0·67 to the thousand living (No. 3). The detailed statistics of Scotland in 1888 (No. 3) bearing on the deaths from cancer show that in the smaller towns the

death-rate from cancer to a "thousand living" is less than in the larger ones, and that in some cities and towns the rate is largely in excess of the rates of neighboring cities and towns.

The annual report of the registrar general of England for 1889 (pp. 13 to 18) contains a discussion on the apparent increase of cancer, from which the following extracts are taken:

"The deaths ascribed to cancer—using that term as a general designation for all malignant new growths—show a further increase upon the ever-growing rates previously recorded. Some of this increase is most certainly attributable to increased accuracy in statement of cause, and to the system introduced some years back of writing for further information in cases where some vague cause, such as 'tumor,' has been given as the cause of death in the original certificate. Nevertheless, in face of the constant and great growth of mortality under this heading, and the expressed belief of medical practitioners specially engaged in dealing with this class of diseases that they are really becoming more and more common, it seems scarcely possible to maintain the optimistic view that the whole of the apparent increase can be thus explained; and it must be admitted, as at any rate highly probable, that a real increase is taking place in the frequency of these malignant affections."

If we examine the cancer statistics of Italy for the year 1889 it will be found that in the less populous portions of this country the death-rate from cancer is less per "thousand living" than in the more populous provinces (No. 4). In the city of Berlin, in 1892, "cancer and tumors" claimed 1,138 of the people, which is 3·48 per cent. of the total deaths of that year. This percentage is the highest ever recorded there from "cancer and tumors." In 1877 a percentage of 2·10 is noted, giving an apparent increase of 2·38 per cent. in fifteen years. The comparative increase by ten-year periods can not be estimated, owing to the incompleteness of the records (No. 5). In 1890 1,009 persons died of cancer in Vienna. This number constituted 4·96 per cent. of her total death-rate for that year. The last half of the sixteen years ending with 1890 shows an increase in the death-rate for cancer of 0·84 per cent.

over the first half of the same period (No. 5). In Paris (No. 6), in 1893, 4·61 per cent. of the total deaths were due to cancer. In Munich (No. 6), in 1892, 3·98 per cent., and in Hamburg, in 1889, 3·78 per cent. of the total deaths were caused by cancer (No. 6). While the percentage is larger in the last three cities, yet no decided change for the worse is noticeable. Permit me now to direct your attention to those cities with which we are all in touch, and therefore we feel for them a personal as well as professional concern. The average death-rate from cancer in New York city during the last ten years is 2·17 per cent. of the total mortality (No. 7), but that of the preceding ten years only 1·82 per cent. Surely these figures are indicative of an increasing mortality from cancer. In the last twenty years 14,369 persons have perished from cancer here, an average of about 718 per annum. It is important to notice that the death-rate from cancer in the State of New York in the years 1885, 1886, and 1887 was 2·37 per cent. of the total death-rate of those periods, and that this rate is 0·20 per cent. greater than the average death-rate of the city of New York during the last decade.* This appears to indicate that the death-rate from cancer in the State at large is greater than in the city. According to a computation made by the late Dr. Carroll, of this city, it appears that the death-rate from cancer in the maritime portions of the State is much less than anywhere else within its limits, and that the greatest death-rate from cancer in the State is in the southern tier of counties. However, if a closer study of these figures be made, and the estimates be based on the death-rate from cancer to the "thousand living," then the strange results of Dr. Carroll's estimates are supplanted by a comparative death-rate in those sections of the State that is in entire conformity with such death-rates elsewhere in the world (No. 9)—that is, greater in the city than in the country districts. In Philadelphia the average death-rate of cancer for the last ten years is 2·24 per cent. of the total mortality, while that of the preceding ten years is 2·28 per cent. (*vide* No. 7). These figures appear to indicate a decrease in the frequency of deaths from cancer, were it not for the fact that during a year of the first ten years (1883)

* *Transactions of the New York State Medical Association.*

the death-rate showed the enormous and strange increase to 3·90 per cent., or nearly 1·5 per cent. above that of any recorded year of that city. However, in 1890 the recorded rate of death from cancer fell to 0·99 per cent. This rate, too, is likewise extraordinary, since it equals only half the percentage of death from cancer in 1876, which year had the smallest rate but one (1875) of the entire twenty years. It seems, therefore, not unfair to exclude these illogical percentages and base the estimate on those of the remaining eighteen years. If this be done, then an increase of 0·26 per cent. during the last nine years is apparent. I will not weary you with further detail, but will state briefly that the record of the last ten years reveals an increase from cancer greater than those of the preceding ten years of 0·54 per cent. in Boston (No. 8), 0·64 per cent. in Baltimore, 0·52 per cent. in New Orleans (No. 11), and 0 84 per cent. in San Francisco (No. 11). These examples are, in my judgment, quite sufficient to establish the fact of the gradual increase of the death-rate from cancer in these localities. It will be noticed that many of the preceding computations are based on the relation of the death-rate from cancer to the total death-rate of each city and country considered. This method gives less accurate results than estimates based on the percentage of deaths from cancer to the "thousand living," owing to the influence on the cancer percentage of the variations in the death-rate due to other causes. However, the uniformity of the death-rate from other causes in the majority of the preceding illustrations does not cause a material change in the result when compared with the latter method of computation. Other cities were asked for records bearing on the question, but as yet they have failed to respond.

The following table is very instructive as bearing on the increase of cancer in the United States:

Year.	Population.	Total deaths.	Deaths from cancer.	Cancer deaths per 100,000 from all causes.	Cancer deaths per 100,000 living.
1850....	23,191,876	323,023	2,088	646	9·0
1860....	31,443,321	394,153	3,672	932	11·7
1870....	38,558,371	492,263	6,224	1,264	16·0
1880....	50,155,783	756,893	13,068	1,815	26·05
1890....	62,622,250	875,521	20,984	.....	33·5

In a private communication, Dr. Billings says, in connection with the record of 1890: "Permit me to call your attention to the fact that for the greater part of the United States the returns of deaths were very incomplete, there being no registration laws. The true death-rate was probably 18 per 1,000, and the death-rate from cancer and tumor per 100,000 living was undoubtedly greater than that above stated" (33·5).

If the attempt be made to compare the death-rate of large cities from this disease with that of the remaining parts of the country, remarkable results are attained. For in fifty large cities 39·94 died from cancer to the 100,000 living; while in the remainder of the country the death-rate from the same cause was 23·60 to the 100,000 living; showing the startling difference between the two of 16·34 to the 100,000 living.

On the other hand, if the percentage of deaths from cancer be compared with deaths from all known causes in both city and country districts, then especially a lowering of the death-rate from cancer in the city is seen, dependent on the large infant mortality from other causes.

If a comparison be made of urban with rural population at the periods of life when cancer is most prevalent, the excess then found is still on the side of the city. This difference is usually too wide to be accounted for entirely by the death-rate in the city of the non-resident afflicted who have sought relief by treatment there. It is, no doubt, true that much of the difference in the death-rates from cancer noted in city and rural populations is dependent on the deaths of the non-residents, who seek individual and hospital aid for the cure of their affliction. This fact is especially noticeable on the continent. In Berlin, from 1883 to 1888, the increase of non-residents in the hospitals of those afflicted with malignant disease was about forty-four per cent. From an official report of Breslau, 1889 to 1892, the following instructive extract is taken:

"The deaths from malignant tumors, which amounted to 14·9 per 1,000 of all deaths in 1877, but gradually increased to 25·4 per 1,000 in 1885, and 32·1 per 1,000 in 1891, shows clearly the effect upon the local death-rates caused by non-residents, whose proportion of the total

deaths of this class was 8·3 per cent. in 1886, 9·8 per cent. in 1887, 11·8 in 1888, 14·4 in 1889, 13·0 in 1890, and 13·2 per cent. in 1891."

It is especially interesting to note the fact that in some portions of the United States cancer is apparently much more prevalent with the rural than the associated urban population; as, for instance, on the north Atlantic coast the rural district shows 31·42 and the urban 22·46 deaths from cancer in 1,000 deaths from all other causes. The middle Atlantic coast gives 20·25 rural to 19·93 of the city; north-eastern hills, 33·23 rural to 17·53 of the city (Worcester and Hartford); south Atlantic coast gives 15·94 rural to 5·70 of the city (Charleston) deaths from cancer to 1,000 from all other causes.

The greater completeness of the mortality records of cities as compared with rural districts accounts for much of the difference in mortality rates from cancer, especially in Charleston, where the number of deaths from infantile diseases in the colored people is very pronounced.

Had these deaths been caused by cholera, or arisen from any other form of human affliction which threatened the safety of the people, or disturbed the avenues of trade, or imperiled the haunts of pleasure, then, indeed, the aggressive resources of the country would have opposed their presence and perpetuation. If the same number of persons were to die annually in these countries as the result of open acute infraction of common law, a knowledge of the fact would furnish texts for innumerable advocates of human redemption, and the basis of princely subscriptions in support of social reform. And, finally, if, despite these measures to establish reform, increased lawlessness were the only response to persistent effort for good, then tender consideration and mercy would be supplanted by vigorous prosecution and punishment.

As a further justification of the combat one need hardly stop now to estimate the importance of the pity of the stranger, the sorrow of the friend, or the anguish of the much-loved in this connection, since these emotions are a part of the life history of all who are endowed with healthy human sentiment.

The logical lines of this unthinking enemy's advance, in a comparative sense, differ in no essential respect from those long since established by the thoughtful study of opposing human agents in their assaults on each other. The vulnerable points are always assailed in human conflicts, and usually they are those of the freest communication.

The lymphatics, lymph nodes, and blood-vessels that are associated immediately with the disease afford easy exit from it of the malign influences that propagate and spread the infection. The malignant products of the original site find free access to contiguous lymph glands through the efferent lymph vessels of a diseased part. The onward march is halted there for a longer or shorter period, long enough at all events to plant the seeds of disease in the gland, which in turn sends on through similar channels to more distant glands its infecting products, and this repeated soon leads to wide infection, to constitutional involvement, and death. It is through these open and easily irrigable channels that malignant influences escape primarily and principally, as from a single spring, and with an ever-broadening and contaminating stream infect finally the entire sea of vitalizing fluid.

As proof of the part played by lymphatics in disease-spread I will call your attention to the results of the analysis of 100 non-hospital cases bearing on this point. In 69 of these cases enlargement of the associated lymphatic glands was present at the time of the primary operation. In 16 cases it is not stated whether they were present or not. In 15 cases only they were not detected by examination. This fact, however, is no proof that they were unaffected, since we frequently find on operation diseased glands infected by contiguous malignant growths that were too small to be appreciated before operation, and in some instances after removal they appeared but slightly enlarged, even though they were then pregnant with active malignant processes. In 94 cases of primary cancer of the tongue the glands were enlarged in eighty-three per cent. of the number. In 34 recurrent cases of this organ glandular enlargement was present in eighty-five per cent. In 118 cases of primary cancer of the breast the axillary

glands were obviously enlarged in seventy-three per cent. of the number. In 42 recurrent cases of the breast the glands were enlarged in eighty-three per cent. of the total number. In 110 instances of non-ulcerated cancer of the breast the axillary glands were enlarged in eighty-three per cent. of the number, while in 50 ulcerated cases these glands were enlarged in ninety-six per cent. of the instances. Surely the inferences to be drawn from these examples bearing on the importance of early and thorough action are too obvious to require comment. One who omits the removal of lymphatic glands as a supplementary step to the excision of malignant disease should present a much better reason for the omission than that no appreciable enlargement of them was noticeable. The rapidity of the flow, the functional activity of the parts, the degree of the infectiousness of the fluid, and the susceptibility of the tissues to its influence, each in some part, or all combined, modify malignant disease dissemination. The blood-vessels, too, act as carriers of infection to every part of the body. Exactly at what period of the early history of a growth the lymphatics and blood-vessels begin the conveyance research has not yet determined.

A careful examination of the hundred cases discloses great paucity of statement regarding the time of the first appearance of enlarged glands, since in four cases only is this feature of development distinctly stated. In these cases two and five months were the extremes.

However, that the lymphatics and blood-vessels are the primary and principal agents of the dissemination of misfortune in malignant disease, and that their contributions to continued infection should be anticipated by an early, diligent, and wise interrogation of tissues possessed of special vulnerability, can not be controverted.

The points of least resistance relate to the immediate environments of the growth. The lymphatic network of vessels whose meshes embrace the tissues on all sides of the growth await with open mouths the opportunity to swallow the local infection. It is then carried by them principally in the direction of the lymph current to direct and more distant places, often poisoning the tissues as it goes along. It is important to remember, if only for conservative pur-

poses, that the infection of tissues against the flow of the lymph current is of a limited degree and of sluggish attainment, while, *per contra*, active local and general infection attends the ebb tide of lymph flow in all parts of the body. Dense tissues with feeble blood supply are comparatively strong defensive barriers against the local assaults of this disease in its onward march of invasion. Loose tissue, abounding in cells and well irrigated with blood, offers an alluring point of attack for malignant invasion, and affords it a fertile field for triumph in the conflict with every natural and artificial reparative agency that opposes its course. The behavior of the resisting and opposing cellular forces in the advance line of conflict is conjectural in a large degree. But of two facts there can be no doubt:

1. That each peculiar cell opposes the advance of its antagonist with a cell vigor suggestive of cell duty and self-preservation. 2. That the healthy cell structures are pushed to the wall, as it were—literally overridden by the rapid advance of the innumerable venom-laden ones bred by the disease—is quite as clearly apparent to the uneducated eye of the layman as to the trained view of the physician.

The sacrifices of defeat, when one is opposed by an obdurate foe, should be sweetened always by the knowledge that one's best has been done. Complete general victory can never be attained in a contest like this, since the ills of the flesh are the heritage of man. But much more will be gained, for years of profit, comfort, and pleasure will come; then the sense of duty better performed will soothe the sorrows of defeat.

The costs of the struggle comprehend great forethought during two or more periods of the conflict—the primary period, the secondary period, etc. To the primary period belong the early detection of the growth; the prompt acknowledgment of its presence; a prompt diagnosis; a prompt and thorough removal of all malignant manifestations. Early detection of a local morbid growth is a matter that relates almost unreservedly to the patient; he should promptly acknowledge its presence to a physician, who should institute at once prompt measures for an accurate diagnosis, and a prompt and thorough removal of all

local disease revelations. Unfortunately for the victim, this series of promptness is rarely evinced. The histories of cases of malignant disease are not bristling with statements that the patients themselves have contributed much at the outset to a knowledge of the presence of a morbid growth, even though they had long recognized the fact that "something wrong" had appeared. Indifference, trepidation, and procrastination are the order of the day, and their benumbing influence on resolution and action have wrecked too frequently the otherwise well-anchored hopes of patients and friends. The indifference is often the result of the belief that nothing of such a nature can "happen to me"—an expression suggestive of the fact that the person regards himself as one more favored by fortune than is his fellow man. Such a belief as this is a selfish faith that often robs one of the gain which follows an earnest, consistent human endeavor. Trepidation leads to incomplete performance of duty, and therefore to the adoption of incomplete methods of preventive procedure. Procrastination puts off until to-morrow the good and the bad alike; both are twin contributors to inaction and adversity.

Perhaps the patient attaches no importance to the growth, or fails to acknowledge its presence for dread of the truth or the folly of vanity. If the physician be told of the unwelcome visitor, he, too, from ignorance, inattention, or sentiment, may dismiss the matter lightly for a while, saying, "Come again in a month or so, and then we'll see." Yet, if he be wise and appreciative of the fact that all tumors are robbers, and, like their kind, may soon destroy the sources of their own enrichment, a prompt removal will quickly follow a prompt, accurate diagnosis.

As bearing on these items, let us examine for a moment the early histories of the hundred cases gleaned from other than hospital sources. It seems to me that such cases as these will illustrate the general trend of thought and action in dealing with malignant growths better than will cases taken from hospital records alone. The extent of the disease at the time of the first recorded professional observation will, I think, constitute one of the proper indices of the degree of promptness exercised in the case.

The series of cases here quoted relate to the tongue and contiguous tissues, and are therefore more important than those of other regions of the body as bearing on the question of prompt action, because the exercise of the functions of these parts must have invited prompt recognition of the presence of a morbid process. Also, cases are selected from this series for the reason that the comparative increase of malignant disease is now greater here in the male than in other portions of the body. In the scheme of investigation, and under the heading "size, contiguous involvement, and situation of the tumor when first seen by the surgeon," appear the following startling examples descriptive of disease advancement: *First case*, "large prominence occupying great portion of the palate"; *second case*, "tumor size of bantam's egg"; *third case*, "large tumor extending into mouth across to right tonsil and causing fullness of the cheek"; *fourth case*, "extensive ulceration of tongue, floor of mouth, and alveolar process"; *fifth case*, "tumor size of orange depending from the soft palate"; *sixth case*, "tumor occupying posterior two fifths of the tongue." Now, when it is recalled that these six illustrations are found in the histories of eighteen consecutive cases taken indiscriminately from the series of one hundred, then, indeed, no stronger argument can be deduced to establish the fact that indifference, trepidation, or procrastination, one or all combined, contributed as much as did any other influence to the sad post-operative prognosis that characterized them. If a closer examination be made of the early history of cases bearing on the question of early recognition and prompt acknowledgment by the patients themselves, the following facts appear: First instance, "when first noticed, tumor size of a pea; thirteen years later, size of an orange." Second instance, when first seen, "size of a cherry"; when seen by the surgeon, "encroached on pillars of the fauces and dorsum of the tongue." Third instance, at first "size of a split pea"; later, "involved pillars of fauces and pharyngeal wall." These statements appear in the histories of twelve consecutive cases taken indiscriminately from the series. The following facts bearing on the time between the discovery of the growth and the period of primary operation are very

instructive: *First case:* "Operated on eighteen years after the first appearance; then greater part of the superior maxilla was removed." *Second case:* "Operation five months after discovery; then portion of lower jaw, large portion of tongue, and floor of mouth excised." *Third case:* "Operation five years after first appearance; returned in three months." *Fourth case:* "Operation fifteen years after first appearance and one year after it was seen by physician." *Fifth case:* "Operation nine months after appearance; then tongue, hyoid bone, and epiglottis were removed." *Sixth case:* "Operation four years after discovery." These six examples of this phase of the subject appear in the histories of fifteen consecutive cases taken at random from the series of one hundred. In order that the hospital aspect of this part of the subject may be noted, I will call your attention to 119 primary cases of cancer of the breast taken from the Middlesex Hospital reports. In these cases it appears that an average of twenty-six months and a half had elapsed between the time the growth was first noticed and the period of admission to the hospital. Twenty-eight of these cases were admitted within six months; 15, six to twelve months; 22, twelve to eighteen months; 13, twenty-four to thirty months; 4, thirty to thirty-six months; 10, thirty-six to forty-two months; 1, forty-two to forty-eight months; 3, forty-eight to fifty-four months; 3, fifty-four to sixty months; 3, sixty to sixty-six months; 4, seventy-two to seventy-eight months; 2, ninety to ninety-six months; 6, over one hundred and two months. In eighteen cases of primary cancer of the lip, taken from the same source, an average period of twenty-four months and a half had elapsed before the patient claimed admission to the hospital. Surely no one could wish for better evidence than the preceding to establish the conclusion that an era of education of patients, and perhaps of the people, is demanded in the interest of a prolonged longevity and cure of those afflicted with malignant disease. If to these conclusions, begotten of recorded facts, be now added the results of similar cases born of the easy recollection of most of the hearers tonight, then, surely, conclusive conviction of the urgent necessity of a campaign of education is doubly empha-

sized. And especially is this true since it further appears that the recorded cases are the product of the last decade.

A thorough, wide removal should always be practiced, and apparently sound tissue logically situated should be freely sacrificed to insure success.

That this proposition is correct, no one who regards malignant disease as local at the outset will have the hardihood to deny. But, even if this be not his belief, he can not fail to recognize the greater value of a healthy reparative post-operative wound, the offspring of a wise and bold removal, when compared with one laden with malignant products, the issue of narrow and timid action. It is difficult indeed to draw reliable conclusions from the results of promiscuous operations, because different operators form diverse estimates of similar cases. For this purpose a clinical or hospital series of cases treated by one who is known to practice bold and thorough methods should be employed. While invidious distinctions will form no part of this lecture, still candor compels me to say that the post-operative records of those surgeons who have the greatest reputation for extensive and wide operative methods disclose the best results in so far as disease recurrence is concerned. Secondary growths and glandular enlargements should be sought for in logical situations and be removed in a similar manner as their antecedents. These growths and enlargements are the vicious progeny of the original malignity, and should meet the same fate as that of their perverse progenitors. Timidity and incompetent examination foster incomplete removal; incomplete removal stimulates the activity of the growth and often too a distrust of the profession that causes the enrichment of quacks. Success at the outset means almost everything. Therefore the patient must be thoughtful and frank; the physician prompt and certain; the surgeon fearless and thorough, and all should then be vigilant, vigilant, vigilant.

The costs of the struggle of the secondary period contemplate the exercise of eternal vigilance, great patience, unselfish devotion, unfaltering determination, and often Christian fortitude on the part of all concerned. The un-

failing employment of intelligent, patient, and periodical scrutiny of the suspected parts by the physician constitutes the vigilance essential for the detection of the earliest reappearance of malignant manifestations. The unselfish devotion of friends and attendants; the unfaltering determination of the surgeon to perform and of the patient to submit promptly to radical measures, require not infrequently the exercise of a degree of fortitude depicted by Milton when he wrote—

> "Nor bate a jot of heart or hope;
> But will still bear up,
> And steer right onward."

And, too, one should not lose sight of the fact that a "faint heart" has the same relation to success with malignant disease that it has with the "fair lady."

The exercise of proper vigilance on the part of the sentinels who are engaged in every anticipated conflict is directed to the common points of renewed attack, and also to the period of expectancy and the aspect of the foe. The common points of renewed attack of malignant disease are at the seat of the primary operation, in the nearest associated lymph glands, and not infrequently the distant indirectly associated parts of the body. The seat of the primary operation is a common point of renewed attack, and the early and wide removal of malignant growths contributes more than any other one element to the failure of reappearance there. If the growth is removed early, it is then small, the infiltration is limited, and the lymphatics are less involved, and too, the resulting deformity is reduced to a minimum; each of which is an important desideratum. In 20 of the series of 100 cases of cancer of the mouth already quoted, the histories of recurrences are quite complete. Of this number 2 recurred at the seat of operation; 7 in glands contiguous to the seat of the primary disease; 8 reappeared simultaneously at the seat of the operation and in the glands; in 3 instances the point of reappearance is not stated. In another series of 14 cases of recurrent cancer following primary disease of the tongue, 5 reappeared in the glands; 8 in the stump of the tongue, and 1 in both tissues simultaneously. Thus we have 34 recurrent cases, taken at large, in which the earliest physical

manifestations of renewed malignancy should have been promptly detected by careful, continued, and logical scrutiny.

The period of expectancy is of very uncertain duration. In the 20 cases before alluded to, 5 reappeared in three months, 2 in two months, 1 in one, four, five, six, nine, sixteen, and twenty months respectively. One is reported to have returned "quickly," another in seven years. This, the last case, attacked the stomach and can not therefore be regarded as a recurrent one in the common usage of the term. In 3 cases the period of recurrence is not stated. One of the cases can not be called recurrent, since the diseased glands present at the primary operation had not been removed, and soon thereafter became the seat of active malignant growth. The period of recurrence is modified in length by the situation, size, nature, and activity of the growth, etc., together with the promptness and thoroughness of removal. Inasmuch as a definite period of expectancy can not be wisely fixed in any case of malignant disease, and therefore as the safety of the patient is secured only by continuous vigil, I will not needlessly encumber the lecture with records of periods easily obtained, and thus, perhaps, disarm wise discretion by the utterance of unreliable dogmatic expressions.

The face of the foe admits of no disguise, for even though it be masked by innocence, its past record of malevolence disarms the possibility of the escape of detection, even if only ordinary prudence be exercised.

In 1889, while considering this subject in a paper before the State association, I made the following assertion: The surgeon's watchword ought to be: *Cut early, cut often, cut wide, and remove contiguous lymphatic structures when practicable, whether diseased or not.* This principle is much older than the expression. Later experience has in no way lessened the force of this statement in my judgment, but has, on the contrary, served to strengthen my belief in the wisdom of the course.

In nearing the termination of this lecture I desire to submit to your consideration the following conclusions:

1. That the death-rate of malignant disease is constantly increasing in all civilized countries.

2. That the death-rate is larger in cities, irrespective of the presence of the transient affected there, than in the country.

3. That in limited portions of the country, and in some cities, the death-rate from malignant disease is strangely and disproportionately large.

4. That this increase of malignant disease demands urgent consideration and prompt logical methods of combat on the part of *all* concerned.

5. That better post-operative prognosis can be attained by proper education of the afflicted and prompter action of the profession.

6. That when the considerations of health and strength shall excite the same degree of forethought and vigilance as the attainment of wealth and power does, then post-operative prognosis will be much improved.

But a few words more in the line of thought of the last conclusion and I am through. The strife for wealth and power on this little planet of ours develops keen and sagacious discernment, vigilant and tireless scrutiny, and sound, aggressive self-reliance, and the people are made better and their belongings more valuable as the result of a proper exercise of these endowments. Now, if it were possible that health be allied with wealth and strength with power in this battle of attainment, then indeed would Pope's sentiment—

> "Reason's whole pleasure, all the joys of sense
> Lie in three words—health, peace, and competence"—

be realized.

No. 1.—England.

Year.	Population.	Total deaths.	Deaths from cancer.	Per cent. of total deaths.	Death-rate from cancer to the 1,000 living.
1865.....	21,145,151	490,909	7,875	1·60	0·37
1866.....	21,409,684	500,689	8,246	1·65	0·38
1867.....	21,677,525	471,073	8,487	1·80	0·39
1868.....	21,948,713	480,622	8,834	1·84	0·40
1869.	22,223,299	494,828	9,266	1·87	0·41
1870	22,501,316	515,329	9,530	1·85	0·42
1871.....	22,788,504	514,879	9,650	1·87	0·42
1872.....	23,096,495	492,265	9,945	2·02	0·43
1873.....	23,408,556	492,520	10,395	2·11	0·44
1874.....	23,724,834	526,632	10,941	2·08	0·46

No 1.—England—(Continued.)

Year.	Population.	Total deaths.	Deaths from cancer.	Per cent. of total deaths.	Death-rate from cancer to the 1,000 living.
1875	24,045,385	546,453	11,336	2·07	0·47
1876	24,370,267	510,315	11,521	2·26	0·47
1877	24,699,539	500,496	12,061	2·41	0·49
1878	25,033,259	539,872	12,594	2·33	0·50
1879	25,371,489	526,255	12,722	2·42	0 50
1880	25,714,288	528,624	13,210	2·50	0·51
1881	26,061,736	491,935	13,542	2·75	0·52
1882	26,413,861	516,654	14,057	2·72	0·53
1883	26,770,744	522,997	14,614	2·79	0·55
1884	27,132,449	530,828	15,198	2·86	0·56
1885	27,499,041	522,750	15,560	2·98	0·57
1886	27,870,586	537,276	16,243	3·02	0·58
1887	28,247,151	530,758	17,113	3·22	0·61
1888	28,628,804	510,971	17,506	3·43	0·61
1889	29,015,613	518,353	18,654	3·60	0·64

No. 2.—London.

1869	3,176,308	77,933	1,584	2·03	0·50
1870	3,221,394	77,634	1,677	2·16	0·52
1871	3,267,251	80,430	1,611	2·00	0·49
1872	3,319,736	71,355	1,712	2·40	0·52
1873	3,373,065	75,459	1,793	2·38	0·53
1874	3,427,250	76,813	1,929	2·51	0·56
1875	3,482,306	81,964	1,951	2·38	0·56
1876	3,538,246	77,671	1,973	2·54	0·56
1877	3,595,085	77,449	1,910	2·47	0·53
1878	3,652,837	84,188	2,086	2·48	0 57
1879	3,711,517	83,964	2,165	2·58	0·58
1880	3,771,139	81,957	2,229	2·72	0·59
1881	3,824,980	81,431	2,358	2·90	0·62
1882	3,862,956	83,015	2,442	2·94	0 63
1883	3,901,309	81,108	2,547	3·14	0·65
1884	3,940,042	82,448	2,623	3·18	0·67
1885	3,979,160	80,978	2,624	3·24	0·66
1886	4,018,666	82,691	2,688	3·25	0·67
1887	4,058,565	82,448	2,874	3·49	0·71
1888	4,098,860	79,244	2,856	3·60	0·70
1889	4,139,555	76,162	2,982	3·92	0·72
1890	4,180,654	89,268	3,258	3·65	0·78
1891	4,222,157	89,122	3,277	3·68	0·78
1892	4,264,076	86,833	3,166	3·65	0·74
1893	4,306,411	91,536	3,412	3·73	0·79

No. 3.—Scotland.

District or Town.	Estimated population (1888).	Total deaths, 1888.	Cancer deaths, 1888.	Per cent. of cancer on total deaths.	Death-rate from cancer to the 1,000 living.
Principal towns	1,588,350	30,505	1,005	3·31	0·63
Large towns	451,982	8,400	267	3·20	0·60
Small towns	853,161	14,284	500	3·53	0·59
Mainland, rural	1,011,419	15,927	610	3·90	0·61
Insular, rural	129,244	2,058	68	3·65	0·58

No. 3.—Scotland.—(Continued.)

District or Town.	Estimated Population (1888).	Total deaths, 1888.	Cancer Deaths, 1888.	Per cent. of cancer on total deaths.	Death-rate from cancer to the 1,000 living.
Glasgow	526,088	11,681	287	2·47	0·55
Edinburgh	262,733	4,738	281	5·94	1·07
Dundee	160,619	2,957	101	3·42	0·63
Aberdeen	119,334	2,179	80	3·68	0·67
Greenock	77,015	1,230	42	3·42	0·55
Leith	74,320	1,175	44	3·76	0·59
Paisley	61,687	1,593	27	1·70	0·44
Perth	32,478	588	20	3·41	0·62
Kilmarnock	27,246	566	27	4·78	0·99
England and Wales	29,405,054	559,684	20,353	3·63	0·69
London	4,264,076	86,833	3,243	3·73	0·76
England and Wales, excluding London.	25,140,978	472,851	17,110	3·62	0·67

These figures for England and London are for the year 1892.

No. 4.—Italy, 1889.

Province or District.	Chief city, with population.	Population of province.	Total deaths.	Cancer deaths.	Per cent. of total deaths.	Death-rate from cancer to the 1,000 living.
Liguria	Genoa (210,652)	940,533	23,371	513	2·23	0·55
Piemonte	Turin (312,422)	3,216,255	77,281	1,167	1·62	0 37
Lombardia	Milan (418,972)	3,881,809	95,858	2,184	2·29	0·56
Veneto	Venice (152,416)	2,966,051	63,864	1,303	2·06	0·44
Lazio	Rome (415,498)	969,602	26,777	471	1·78	0·49
Campania	Naples (517,067)	3,028,927	78,728	960	1·24	0·32
Toscana	Florence (185,057)	2,266,933	56,167	1,667	3·00	0·74
Emilia	Bologna (138,339).	2,245,358	58,705	1,492	2·57	0·66
Marche	.....	959,010	24,904	560	2·28	0·58
Umbria	.....	590,865	15,385	273	1·79	0·46
Abruzzi e Molise	.....	1,355,582	37,851	385	1·06	0·28
Puglie	.....	1,740,470	49,341	495	1·01	0·28
Basilicata	.....	537,129	14,985	122	0·84	0·23
Calabria	.....	1,303,813	34,666	310	0·95	0 24
Sicilia	Messina (139,744). Palermo (266,872)	3,245,742	90,446	898	1·02	0·28
Sardegna	.....	721,575	19,739	123	0·72	0·17

No. 5.—Berlin.

Year.	Population.	Total deaths.	Deaths from cancer.	Per cent. of total deaths.	Death-rate from cancer to the 1,000 living.
1874	916,449	27,286			
1875	948,534	31,225			
1876	981,118	29,185			
1877	1,010,946	29,988	631	2·10	0·62
1878	1,039,447	30,629			
1879	1,071,886	29,545			

No. 5.—Berlin.—(Continued.)*

Year.	Population.	Total deaths.	Deaths from cancer.	Per cent. of total deaths.	Death-rate from cancer to the 1,000 living.
1880	1,106,339	32,823	736	2·24	0·67
1881	1,140,001	31,055	584	1·88	0·51
1882	1,174,216	30,465	639	2·10	0·54
1883	1,209,159	35,056	668	1·91	0·55
1884	1,250,895	32,932	795	2·41	0·64
1885	1,291,359	31,483	724	2·30	0·56
1886	1,339,108	34,293			
1887	1,389,841	30,333			
1888	1,443,742	29,294	943	3·22	0·65
1889	1,496,344	34,459			
1890	1,553,665	33,393	1,032	3·09	0·66
1891	1,601,327	33,392	1,091	3·27	0·68
1892	1,637,283	32,696	1,138	3·48	0·69
		Vienna.†			
1874	649,931	19,926	649	3·26	1·00
1875	658,763	20.418	702	3·44	1·07
1876	667,715	21,556	699	3·24	1·05
1877	676,789	20,935	704	3·36	1·04
1878	685,986	21,643	720	3·33	1·05
1879	695,308	21,062	770	3·66	1·11
1880	704,756	20,453	729	3·56	1·03
1881	715,257	21,549	804	3·73	1·12
1882	725,935	21,595	799	3·70	1·10
1883	736,773	21,194	871	4·11	1·18
1884	747,772	20,353	910	4·47	1·22
1885	758,935	21,976	916	4·17	1·21
1886	770,265	20,869	918	4·40	1·19
1887	781,764	20,549	911	4·43	1·17
1888	793,434	20,349	942	4·63	1·19
1889	805,278	20,106	1,060	5·25	1·32
1890	817,299	20,324	1,009	4·96	1·23
		No. 6.—Paris.‡			
1886	2,260,945	57,092			
1887	2,292,787	54,847	2,350	4·28	1·02
1888	2,325,078	53,303	2,423	4·55	1·04
1889	2,357,824	56,059	2,461	4·39	1·04
1890	2,391,031	56,660	2,445	4·32	1·02
1891	2,424,705	54,443	2,525	4·64	1·04
1892	2,458,853	57,137	2,459	4·30	1·00
1893	2,493,483	55,469	2,557	4·61	1·03

* Figures taken from official annual reports, in which there is no separate heading for cancer. The deaths in the cancer column, as given above, are "Krebs und Geschwülste"—that is, "cancer and tumors."

† Figures taken from official annual reports, in which cancer is given by itself.

‡ Populations for 1886 and 1891 are the census figures. Other years have been calculated by the common formula for geometrical annual increase, as the Paris officials do not make any estimates for intercensal years, but use the figures of one census until they have another.

*Munich.**

Year.	Population.	Total deaths.	Deaths from cancer.	Per cent. of total deaths.	Death-rate from cancer to the 1,000 living.
1883.....	*a*230,000	7,676	225	2·93	0·98
1884.....	240,000	7,369	294	3·99	1·22
1885.....	*a*251,000	7,539	276	3·66	1·10
1886.....	262,000	7,847	295	3·76	1·13
1887.....	*a*268,000	8,057	338	4·20	1·26
1888.....	275,000	8,236	315	3·82	1·15
1889.....	281,000	8,721	395	4·53	1·41
1890.....	349,024	8,953	307	3·43	0·88
1891.....	357,000	9,854	347	3·52	0·97
1892.....	373,000	9,717	387	3·98	1·04

Hamburg (State). †

1884.....	500,273	12,577	488	3·88	0·98
1885.....	513,224	12,983	475	3·66	0·93
1886.....	525,150	15,247	519	3·40	0·99
1887.....	538,750	14,350	522	3·64	0·97
1888.....	559,290	13,735	522	3·80	0·93
1889.....	584,080	13,584	513	3·78	0·88

No. 7.—New York.

1874.....	1,030,607	28,727	416	1·45	0·40
1875	1,044,396	30,709	423	1·38	0·40
1876.....	1,075,532	29,152	459	1·57	0·43
1877.....	1,107,597	26,203	495	1·89	0·45
1878.....	1,140,617	27,008	570	2·11	0·50
1879.....	1,174,621	28,342	572	2·02	0·49
1880.....	1,209,709	31,937	659	2·06	0·54
1881.....	1,252,017	38,624	706	1·83	0·56
1882.....	1,295,805	37,924	732	1·93	0·56
1883.....	1,341,124	34,011	678	1·99	0·51
1884.....	1,388,028	35,034	731	2·09	0·53
1885.....	1,436,572	35,682	754	2·11	0·52
1886.....	1,486,814	37,351	779	2·09	0·52
1887.....	1,538,814	38,933	832	2·14	0·54
1888.....	1,592,632	40,175	870	2·17	0·55
1889.....	1,648,332	39,679	848	2·14	0·51
1890.....	1,705,980	40,103	954	2·38	0·56
1891.....	1,765,645	43,659	902	2·07	0·51
1892.....	1,827,396	44,329	996	2·25	0·55
1893.....	1,891,306	44,486	993	2·23	0·53

* Populations marked *a* were inserted by the compiler; the others are all from official reports, the figures for 1890 being census figures apparently, and the others mere guesswork.

† Figures taken from the "Statistisches Handbuch für den Hamburgischen Staat, 1891."

Of every 100,000 living died, on the average, from cancer, 1879–'83, 88·7; 1884–'89, 94·4. *Op. cit.*, p. 57.

Philadelphia.

Year.	Population.	Total deaths.	Deaths from cancer.	Per cent. of total deaths.	Death-rate from cancer to the 1,000 living.
1874	775,000	15,238	307	2·01	0·40
1875	800,000	17,805	317	1·78	0·40
1876	825,594	18,892	373	1·97	0·45
1877	850,856	16,004	327	2·04	0 38
1878	876,118	15,743	380	2·41	0·43
1879	901,380	15,473	364	2·35	0·40
1880	846,980	17,711	368	2·08	0·43
1881	868,000	19,515	413	2·12	0·48
1882	886,539	20,059	429	2·14	0·48
1883	907,041	20,076	* 783	3·90	0·86
1884	927,995	19,999	476	2·38	0·51
1885	949,432	21,392	487	2·28	0·51
1886	971,363	20,005	461	2·30	0·47
1887	993,801	21,719	500	2·30	0·50
1888	1,016,758	20,372	448	2·20	0·44
1889	1,040,245	20,536	531	2·59	0·51
1890	1,046,964	21,732	* 215	0·99	0·21
1891	1,069,264	23,367	572	2·45	0·53
1892	1,092,168	24,305	569	2·34	0·52
1893	1,115,562	23,655	614	2·60	0·55

No. 8.—Population, Deaths, and Death-rates from Cancer for Twenty Years. Boston, Mass.

1874	313,745	7,812	140	1·79	0·45
1875	360,122	9,060	156	1·72	0·43
1876	352,842	8,253	164	1·99	0·46
1877	363,000	7,316	193	2·64	0·53
1878	363,000	7,636	196	2·57	0·54
1879	363,000	7,398	195	2·64	0·54
1880	362,839	8,531	214	2·51	0·59
1881	397,628	9,016	241	2·67	0·61
1882	410,376	8,995	253	2·81	0·62
1883	427,940	9,740	293	3·01	0·68
1884	427,940	9,622	282	2·93	0·66
1885	400,000	9,618	274	2·85	0·68
1886	400,000	9,268	299	3·23	0·75
1887	400,000	10,073	324	3·22	0·81
1888	415,000	10,197	279	2·74	0·67
1889	420,000	10,259	306	2·98	0·73
1890	448,477	10,181	326	3·20	0·73
1891	459,062	10,571	317	3·00	0·69
1892	469,647	11,236	328	2·92	0·70
1893	487,397	11,710	310	2·65	0·64

* Note by health officer of Philadelphia: "Very extraordinary mortality, not explained. In 1890 the influenza epidemic may have carried off many already weakened by disease, which would accountfor the decreased mortality"—J. V. R. Turner, City Registrar Clerk.

*No. 9.—Baltimore.**

Year.	Population.	Total deaths.	Deaths from cancer.	Per cent. of total deaths.	Death-rate from cancer to the 1,000 living.
1874	267,354	7,401	83	1·12	0·31
1875	355,000	7,317	127	1·74	0·36
1876	355,000	7,382	122	1·65	0·34
1877	360,000	7,936	141	1·78	0·39
1878	360,000	6,733	177	2·63	0·49
1879	375,000	7,618	151	1·98	0·40
1880	375,000	8,043	155	1·93	0·41
1881	393,000	8,816	175	1·98	0·45
1882	408,520	8,823	163	1·85	0·40
1883	408,520	9,380	163	1·74	0·40
1884	408,520	8,293	182	2·19	0·45
1885	417,220	8,153	185	2·27	0·44
1886	417,220	8,339	205	2·46	0·49
1887	437,155	8,372	220	2·63	0·50
1888	437,155	8,936	226	2·53	0·52
1889	437,155	8,703	226	2·60	0·52
1890	455,427	10,198	276	2·71	0·61
1891	455,427	10,073	267	2·65	0·59
1892	455,427	10,582	232	2·19	0·51
1893	455,427	9,554	249	2·61	0·55

*No. 10.—New Orleans.**

1874	210,000	6,798	88	1·29	0·42
1875	210,000	6,117	89	1·46	0·42
1876	210,000	6,257	91	1·45	0·43
1877	210,000	6,708	113	1·68	0·54
1878	210,000	10,318	115	1·11	0·55
1879	210,000	5,122	105	2·05	0·50
1880	216,090	5,623	129	2·29	0·60
1881	218,879	6,406	127	1·98	0·58
1882	221,704	5,922	143	2·41	0·65
1883	224,556	7,523	122	1·62	0·54
1884	227,465	7,150	151	2·11	0·66
1885	234,000	6,670	135	2·02	0·58
1886	238,000	6,291	157	2·50	0·66
1887	242,750	6,075	143	2·35	0·60
1888	248,000	6,301	136	2·16	0·55
1889	254,000	6,075	139	2·29	0·55
1890	254,000	7,238	187	2·58	0·74
1891	254,000	6,856	152	2·22	0·60
1892	254,000	7,499	160	2·13	0·63
1893	254,000	7,556	167	2·21	0·66

No. 11.—San Francisco.

1874	230,132	3,710	63	1·70	0·27
1875	272,345	4,189	88	2·10	0·32
1876	300,000	5,555	97	1·75	0·32
1877	300,000	4,977	129	2·59	0·43
1878	305,000	4,493	117	2·60	0·38

* Figures furnished, in response to inquiry, by the health department.

No. 11.—San Francisco.—(Continued.)

Year.	Population.	Total Deaths.	Deaths. from cancer.	Per cent. of total deaths.	Death-rate from cancer to the 1,000 living.
1879.....	234,520	4,340	122	2·81	0·52
1880.....	234,520	4,287	109	2·54	0·46
1881.....	234,520	5,008	148	2·96	0·63
1882.....	250,000	4,988	152	3·05	0·61
1883.....	270,000	5,000	164	3·28	0·61
1884.....	270,000	5,288	138	2·61	0·51
1885.....	280,000	5,322	180	3·38	0·64
1886.....	300,000	5,359	184	3·43	0·61
1887.....	330,000	6,036	190	3·15	0·58
1888.....	330,000	5,729	195	3·40	0·59
1889.....	300,000	6,378	216	3·39	0·72
1890.....	330,000	6,650	243	3·65	0·74
1891.....	330,000	6,911	230	3·33	0·70
1892.....	330,000	6,061	243	4·01	0·74
1893.....	330,000	6,060	210	3·47	0·64

SCIENTIFIC WORK

IN THE

SECTIONS OF THE ACADEMY.

SECTION ON GENERAL MEDICINE.

JANUARY 16TH, 1894.—J. Daland, M. D., of Philadelphia. Paper: "Value of the haematokrit in the diagnosis of anæmia," with demonstration. Published: Jour. Acad., Feb. 15th, 1895. Discussed by Drs. W. G. Thompson, R. W. Wilcox, Smith, and J. M. Byron.

MARCH 20TH, 1894.—A. Zeh, M. D. Paper: "The dietetic and therapeutic management of typhoid fever." Published: Amer. Med. Surg. Bull., 1894, vii., 463. Discussed by Drs. W. H. Thomson, A. M. Lesser, Max Einhorn, L. F. Bishop, C. E. Quimby.

Exhibition and demonstration of a new portable bath-tub devised by Dr. A. R. Bellamy.

APRIL 17TH, 1894.—J. N. Henry, M. D., presented a case of chyluria, and exhibited a photograph of the filaria sanguinis hominis obtained from the patient's blood.

J. W. Brannan, M. D., presented a case of myelogenous or spleno-myelogenous leuchæmia; and a case of arterio-fibro-sclerosis.

T. S. Southworth, M. D. Paper: "The colorless corpuscles of the blood." Published: Amer. Med. Surg. Bull., 1894, vii., 585. He also presented a case of leuchæmia. He also demonstrated with the microscope various forms of colorless corpuscles.

G. R. Lockwood, M. D., reported a case of rapidly fatal leuchæmia.

MAY 15TH, 1894.—J. C. Smith, M. D., presented a case of myxœdema. Discussed by M. Allen Starr, M. D.

L. D. P. Clark, M. D. Paper: "Some observations on an epidemic of typhus fever." Published: Amer. Med. Surg. Bull., 1894, vii., 647.

W. H. Park, M. D. Remarks: "The bacteriology of typhus fever."

Further remarks by Drs. C. E. Bruce and W. H. Porter.

OCTOBER 16TH, 1894.—M. Manges, M. D., presented a case of sporadic cretinism.

G. W. Crary, M. D., spoke on "The treatment and frequency of cretinism."

F. C. Williams, M. D. (of Boston, Mass.) Paper: "Observations on the diagnosis of diphtheria and a study of its local treatment." Published: Amer. Jour. Med. Sci., 1895, cx., 37. Discussion by Drs. G. L. Peabody, A. Caillé, W. H. Park, J. E. Winters, C. White, and A. Jacobi.

NOVEMBER 20TH, 1894.—G. P. Biggs, M. D., presented a series of 20 specimens illustrating the varieties of biliary calculi.

M. Manges, M. D., presented three specimens of biliary calculi.

T. S. Southworth, M. D. Paper: "A résumé of the subject of biliary calculi." Published: Med. Rec., 1894, xlvi., 749. Discussion by Drs. F. P. Kinnicutt, W. G. Thompson, B. Robinson, R. Abbe, and L. Weber.

DECEMBER 18TH, 1894.—R. W. Wilcox, M. D. Paper: "The use of the vaso-constrictors in cardiac disease." Published: Med. Rec., 1895, xlvii., 141. Discussion by Drs. C. E. Quimby, W. H. Porter, and W. M. H. McEnroe.

W. G. LEBOUTILLIER,
Secretary.

SECTION ON GENERAL SURGERY.

JANUARY 8TH, 1894.—C. A. Powers, M. D. Paper: "Further observations on the physiological occurrence of conical stump after amputations in children." Published: Med. Rec., 1894, xlv., 423. Discussed by Drs. Whitman, Lilienthal, Van Arsdale, and Bryant.

J. P. Tuttle, M. D. Paper: "The modern treatment of hæmorrhoids." Published: N. Y. Med. Jour., 1894, lix, 518. Discussed by Drs. Meyer, Lloyd, Kammerer, Manley, Bryant, and Tuttle.

Patients were presented as follows:

F. Kammerer, M. D. Inguinal hernia, cured by McEwen's operation.

F. Kammerer, M. D. Gastro-enterostomy for cancer of the stomach.

W. W. Van Arsdale, M. D. A case of conical stump of the upper extremity in a child of eight years; twice amputated for a similar condition.

H. L. Shively, M. D. Conical stump of the upper extremity of a child.

F. Kammerer, M. D., presented a pathological specimen of gastro-enterostomy for cancer of the stomach.

W. Meyer, M. D., showed a portable sterilizer for private operation. Published: Med. Rec., 1894, xlv., 285.

FEBRUARY 12TH, 1894.—W. T. Bull, M. D. Paper: "Further observations on chronic relapsing appendicitis." Published: Med. Rec., 1894, xlv., 385. Discussed by Drs. Abbe, Bryant, Kammerer, Meyer, Edebohls, Morris, and Bull.

Patients were presented as follows:

R. Abbe, M. D. Chronic relapsing œdema of the

right lower extremity in a young woman. Discussed by Drs. Bryant, Kammerer, R. H. Sayre, and Abbe.

W. Meyer, M. D. Disorganization of the left shoulder-joint, the result of syringo-myelia. Discussed by Drs. Bryant, Meyer, and Powers.

W. Meyer, M. D. Disorganization of a metacarpo-phalangeal joint, the result of locomotor ataxia.

Pathological specimens presented:

J. D. Bryant, M. D. Round cell sarcoma from the tongue. Adeno-sarcoma of the breast. Adeno-fibroma of the breast. Discussed by Drs. Dowd and Tuttle.

FEBRUARY 27TH, 1894.—H. Lilienthal, M. D. Paper: "A method of differential diagnosis in œsophageal stricture, with the report of a case." Published: N. Y. Med. Jour., 1894, lix., 496. Discussed by Dr. Torek.

A. M. Phelps, M. D. Paper: "A new operation for the radical cure of inguinal hernia." Published: N. Y. Med. Jour., 1894, lx., 291. Discussed by Drs. Bryant, Kammerer, Dawbarn, Coley, Manley, Walker, Powers, and Phelps.

Patients were presented as follows:

H. Lilienthal, M. D. Compound fracture of the olecranon process, treated by secondary suture with buried silver wire.

C. Beck, M. D. Operation for the cure of a congenital fistula of the neck. Published: Med. Rec., 1894, xlv., 335. Discussed by Dr. Kelly.

F. Kammerer, M. D. Twenty patients illustrating the Bassini method for the radical cure of inguinal hernia. Discussed by Drs. Lilienthal, Kammerer, Curtis, Bryant, and Coley.

F. Kammerer, M. D. Compound fracture near the elbow-joint treated by secondary suture.

F. Kammerer, M. D. Venous angioma of the hand, cured by operation.

R. T. Morris, M. D. Demonstration of the production of iliac intussusception by carbonate of sodium.

MARCH 12TH, 1894.—C. N. Dowd, M. D. Paper: "Considerations on the different types of surgical inflammation, based on bacterial examinations from 125 cases." Published: Med. Rec., 1894, xlvi., 289. Discussed by Drs. Park, Biggs, Coley, Van Arsdale, Morris, Erdmann, Gibson, Powers, and Dowd.

W. Meyer, M. D. Paper: "Practical and efficient sterilization of the materials for private operations." Published: Med. Rec., 1894, xlv., 587.

Patients were presented as follows:

C. Beck, M. D. Extensive epitheliomatous ulceration of the forehead in a woman. Discussed by Dr. Erdmann.

F. Kammerer, M. D. Extensive osteosarcoma of the pelvis in a young man. Discussed by Drs. Powers and Coley.

Exhibition of apparatus:

W. Meyer, M. D. A portable sterilizer.

Mr. Van Husen. A portable sterilizer.

W. Meyer, M. D. An apparatus for the sterilization of catgut.

J. B. Walker. Box containing small bottles of catgut and silk.

The subject of sterilization, etc., was discussed by Drs. Gallant, Stuart, Jones, Kammerer, Dunham, Biggs, Van Arsdale, Coley, Agramonte, Powers, Meyer, and Mr. Van Husen.

APRIL 9TH, 1894.—J. B. Walker, M. D. Paper: "Rupture of the quadriceps extensor muscle and its tendon: an analysis of 252 cases with reference to treatment and results." Discussed by Drs. Powers and Meyer.

Introductory remarks on the subject of cleansing the field of operation were made by Dr. J. Wright, on the mouth, nose, and pharynx; by Dr. W. K. Otis, on the urethra and bladder, and of the instruments used. Discussed by Drs. Tuttle, Coley, Dowd, Agramonte, and Morris.

Patients were presented as follows:

A. M. Phelps, M. D. A case of extensive resection of the wrist and bones of the forearm for tuberculous abscess.

Also: Removal of the scapula for fibroma. Discussed by Dr. Powers.

Also: A case of hip disease wearing the Phelps brace.

W. Meyer, M. D. A case of pylorectomy with gastro-duodenostomy by the Murphy button. Discussed by Drs. Einhorn, Meyer, and Powers.

Exhibition of apparatus:

Dr. Morris. A new urethroscope. Discussed by Drs. Meyer, Otis, Goldenberg, Brown, Lilienthal, and Morris.

MAY 14TH, 1894. J. D. Bryant, M. D. Paper: "The influence of physiological rest on prolapse of the rectum." Published: Matthews Med. Quart., 1894, i., 513. Discussed by Drs. Gallaudet, Abbe, Tuttle, Murphy of Chicago, Erdmann, Lilienthal, and Bryant.

G. R. White, M. D. Paper: "Malignant oedema." Discussed by Drs. Abbe and Gibson.

Patients were presented as follows:

V. P. Gibney, M. D. Hoppa's operation for congenital dislocation of the hip.

Also: Operation for flail hip.

Also: Reduction of traumatic dislocation of the hip by the open method.

Also: Elongation of the ligamentum patellae. Discussed by Drs. Whitman and Gibney.

R. Abbe, M. D. A modification of the technique ot skin grafting.

B. B. Gallaudet, M. D. Two cases of noma; one treated by a plastic operation. Discussed by Drs. Bryant, Powers, and Gallaudet.

R. Whitman, M. D. Removal of the sheath of the extensor tendons of the hand for tuberculous disease. Discussed by Dr. Bryant.

OCTOBER 8TH, 1894.—C. N. Dowd, M. D. Paper: "A report of two cases of multiple subcutaneous tumors."

Patients were presented as follows:

A. M. Phelps, M. D. The radical cure of inguinal hernia by the "wire mattress" method. Discussed by Dr. Hopkins.

R. Whitman, M. D. Two cases of "bending of the neck of the femur;" one single and the other double deformity.

B. F. Curtis, M. D. "Bending of the neck of the femur." The cases discussed by Drs. Sayre, Allen, Curtis, and Whitman.

J. Teschner, M. D. Fracture of the forearm treated by extension. Discussed by Drs. Curtis and Teschner.

W. Meyer, M. D. Osteosarcoma of the ilium treated by the injection of the toxins of erysipelas. Discussed by Drs. Shrady, Curtis, Syms, and Coley.

W. Meyer, M. D. Resection of the knee-joint for Charcot's disease. Discussed by Drs. Syms and Curtis.

A. M. Phelps, M. D. Exhibition of apparatus. A modification of the Grattan osteoclast.

NOVEMBER 2D, 1894.—F. H. Markoe, M. D. Paper: "A clinical contribution to the surgery of intestinal obstruction." Published: Med. Surg. Bull., 1894, vii., 1425. Discussed by Drs. Tuttle, Morris, DeGarmo, and Markoe.

W. Meyer, M. D. Paper: "An improved method of the radical operation of carcinoma of the breast." Published: Med. Rec., 1894, xlvi., 746. Discussed by Drs. Boldt, Manley, Dawbarn, Gallant, Fowler, Lloyd, Markoe, Van Arsdale, and Meyer.

Patients were presented as follows:

S. Tousey, M. D. Removal of a lead-pencil from the axilla, which had been mistaken for fracture of the clavicle. Discussed by Drs. Markoe and Tousey.

H. Lilienthal, M. D. Excision of the colon for carcinoma, with end to end anastomosis by the Murphy button. Discussed by Drs. Meyer, Van Arsdale, and Lilienthal.

W. W. Van Arsdale, M. D. Excision of the rectum by the Kraske method.

Exhibition of specimens:

C. L. Gibson, M. D. Gall stones with remarks on the operation of "Ideal Cholecystotomy." Discussed by Drs. Van Arsdale, Lilienthal, Dawbarn, Meyer, and Gibson.

DECEMBER 10TH, 1894.—R. T. Morris, M. D. Paper: "Cases of appendicitis treated by the inch and a half incision." Discussed by Drs. McBurney, Bryant, Wyeth, Stimson, Wiggin, Curtis, and Morris.

Patients were presented as follows:

R. T. Morris, M. D. Six patients illustrating the treatment of appendicitis by the inch and a half incision.

J. A. Wyeth, M. D. Subclavian innominate aneurism.

Also: Dilatation of the aorta. Discussed by Dr. Bryant.

J. D. Bryant, M. D. Rhinoplasty by lateral flaps. Discussed by Drs. Wyeth, Curtis, and Bryant.

F. S. Dennis, M. D. End to end anastomosis by the Murphy button. Discussed by Drs. Wyeth, Wiggin, Dawbarn, and Gallant.

F. Kammerer, M. D. Excision of the superior dental nerve.

H. Lilienthal, M. D. Traumatic meningocele.

R. Whitman, M. D. A case of conical stump.

Exhibition of specimen:

F. S. Dennis, M. D. Specimen illustrating the various methods of intestinal suture.

ROYAL WHITMAN, M. D.,
Secretary.

SECTION ON GENITO-URINARY SURGERY.

JANUARY 9TH, 1894.—F. T. Brown, M. D., presented a case of syphilitic exostosis of the frontal bone. Discussed by Drs. G. E. Brewer, C. W. Allen, J. A. Fordyce, J. P. Tuttle, R. W. Taylor, and F. T. Brown.

C. W. Allen, M. D. Paper: "Gangrene of the scrotum." Published: Jour. Cut. and Gen.-Urin. Dis., 1894, xii., 55. Discussed by Drs. J. A. Fordyce, F. T. Brown, T. H. Manley, E. Fuller, R. W. Taylor, and C. W. Allen.

FEBRUARY 13TH, 1894.—B. E. Vaughan, M. D., reported two cases of multiple chancre.

C. L. Gibson, M. D., showed specimen of cancer of penis, and related the history of a case of epithelioma of the penis.

R. W. Taylor, M. D. Paper: "Cancer of the penis and groin." Discussed by Drs. E. Fuller, J. A. Fordyce, B. E. Vaughan, C. A. Powers, F. T. Brown, W. K. Otis, C. W. Allen, S. Alexander, T. H. Manley, Vanderpoel, and R. W. Taylor.

MARCH 13TH, 1894.—F. T. Brown, M. D., exhibited a

suspensory clamp; also a modification of the Clover crutch for perineal operations.

W. G. Le Boutillier, M. D., presented a specimen of tuberculosis of bladder and kidneys.

R. F. Weir, M. D., reported a case of vesical tumor and showed specimen.

L. B. Bangs, M. D., presented specimen of recurrent epithelioma of bladder.

F. T. Brown, M. D. Paper: "Posterior urethritis." Discussed by Drs. E. M. Culver, G. E. Brewer, R. W. Taylor, G. K. Swinburne, W. K. Otis, E. Fuller, H. G. Klotz, and J. P. Tuttle.

R. Guiteras, M. D. Paper: "Periurethral phlegmon near the frenum." Published: Jour. Cut. and Gen.-Urin. Dis., 1894, xii., 253. Discussed by Drs. G. E. Brewer and W. K. Otis.

April 10th, 1894.—R. W. Taylor, M. D., presented some vesical calculi.

J. R. Hayden, M. D., presented a renal calculus. Discussed by Drs. W. G. LeBoutillier, H. Goldenberg, F. T. Brown, and B. E. Vaughan.

C. Beck, M. D., presented two patients upon whom he had performed lithotomy.

W. G. LeBoutillier, M. D., reported two cases of syphilis from tattooing.

R. Guiteras, M. D., presented a patient with artificial testicle.

B. E. Vaughan, M. D., presented a case of tuberculosis of the prostate. Discussed by Drs. E. Fuller, B. E. Vaughan, G. E. Brewer, and W. K. Otis.

G. H. Fox, M. D. Paper: "Some common mistakes in the treatment of syphilis." Discussed by Drs. C. W. Allen, G. E. Brewer, E. B. Bronson, P. A. Morrow, J. A. Fordyce, E. Fuller, and W. K. Otis.

May 8th, 1894.—Discussion on Dr. Fox's paper, continued by Drs. H. G. Klotz, R. W. Taylor, B. E. Vaughan, and G. H. Fox.

C. L. Gibson, M. D., showed a patient who had been castrated for tubercular epididymitis.

R. W. Taylor, M. D., presented a mixed malignant growth of the testicle. Discussed by Dr. E. L. Keyes.

H. Goldenberg, M. D., presented a case for diagnosis. Discussed by Drs. R. W. Taylor, E. Fuller, S. Alexander, and E. L. Keyes.

R. Guiteras, M. D., presented a case for diagnosis. Discussed by Drs. C. W. Allen, C. L. Gibson, J. A. Fordyce, E. L. Keyes, S. Alexander, E. Fuller, R. W. Taylor, H. Goldenberg, and R. Guiteras.

F. F. Smith, M. D., reported a case of castration for hypertrophy of prostate. Published: Annal. Surg. 1894, xx., 52. Discussed by Drs. E. L. Keyes, D. Alexander, E. Fuller, R. Guiteras, C. W. Allen, R. W. Taylor, H. Goldenberg, and Smith.

J. A. Fordyce, M. D., showed a case of miliary papular syphilide in a negro.

C. L. Gibson, M. D. Paper: "Castration for tubercular testis." Discussed by Drs. S. Alexander, E. L. Keyes, E. Fuller, R. W. Taylor, and Gibson.

OCTOBER 9TH, 1894.—G. E. Brewer, M. D., reported two cases of vesical neurosis. Discussed by Drs. F. T. Brown, E. Fuller, B. E. Vaughan, and R. W. Taylor.

F. T. Brown, M. D., reported a case of gonorrhea aborted by nitrate of silver. Discussed by Drs. G. E. Brewer, E. Fuller, H. G. Klotz, B. E. Vaughan, R. W. Taylor, and Brown.

B. E. Vaughan, M. D., reported a case of ulcer of the bladder. Discussed by Drs. E. Fuller, F. T. Brown, R. W. Taylor, J. P. Tuttle, and Vaughan.

L. B. Bangs, M. D., presented a case of hydronephrosis from obstruction.

NOVEMBER 13TH, 1894.—B. F. Curtis, M. D., presented a case of hypospadias cured by plastic operation.

F. T. Brown, M. D., reported a case of fibro-cystic tumor of the scrotum. Discussed by Drs. Goldenberg and Brown.

H. Goldenberg, M. D. Paper: "Posterior urethritis and the diagnostic value of the modified Thompson test." Published: Jour. Cut. and Gen.-Urin. Dis., 1894, xii., 518. Discussed by Drs. R. W. Taylor, E. L. Keyes, H. G. Klotz, G. H. Swinburne, E. Fuller, J. P. Tuttle, S. Alexander, and Goldenberg.

Drs. Vaughan and Brooks. Paper: "Study of the gonococcus." Published: Jour. Cut. and Gen.-Urin. Dis., 1895, xiii., 18. Discussed by Drs. R. W. Taylor, and H. Heiman.

DECEMBER 11TH, 1894.—F. T. Brown, M. D., presented a case of urethro-rectal fistula. Discussed by Drs. E. Fuller, and Debout-d'Estrées.

W. Meyer, M. D. Paper: "Further experience in the effect of simultaneous ligation of both internal iliac arteries for hypertrophy of the prostate gland." Published: Annal. Surg., 1896, xxiii., 705. Discussed by Drs. S. Alexander, E. Fuller, C. W. Allen, and Meyer.

R. Guiteras, M. D., showed a catheter plug.

J. P. Tuttle, M. D. Paper: "Extreme local dilatation in the treatment of urethral stricture and its allied conditions." Published: N. Y. Med. Jour., 1895, lxi., 449. Discussed by Drs. C. W. Allen, R. Guiteras, H. G. Klotz, R. W. Taylor, and Tuttle.

J. R. HAYDEN,
Secretary.

SECTION ON ORTHOPÆDIC SURGERY.

JANUARY 19TH, 1894.—Demonstration of Shaffer's operation for the relief of rotary lateral curvature. Discussed by Drs. Phelps, Ketch, Haynes, Gibney, Myers, Whitman, Sayre, B. F. Curtis, and W. R. Townsend.

FEBRUARY 17TH, 1894.—R. Whitman, M. D., presented a case of rickets with marked knock-knee and backward bowing of the tibia. Discussed by Drs. Townsend, Gibney and Sayre.

L. W. Hubbard, M. D., presented a case of lumbar Pott's disease, showing nervous symptoms. Discussed by Drs. Whitman, Townsend, Sayre, Taylor, and Berg.

S. Lloyd, M. D., presented a case of evolution of the second toe to take the place of the great toe in a case in which he had performed Thiersch's skin grafting to cover an ulcer in the cicatrix following the amputation of the great toe.

T. H. Myers, M. D., presented the histories of two cases of chronic hydrocephalus. Discussed by Drs. Gibney and Townsend.

V. P. Gibney, M. D., presented a specimen of a tubercular focus in the head of the femur, also a case of removal of a portion of the ulna to overcome deformity resulting from an injury to the radius, and one of cuneiform osteotomy for deformity following hip disease. Discussed by Dr. Hubbard.

MARCH 16TH, 1894.—F. Churchill, M. D., presented a case of Pott's disease which had had several attacks of paraplegia with dislocation of the right hip. Discussed by Drs. Gibney, Sayre, and Berg.

V. P. Gibney, M. D., presented a case of double genu valgum with lateral curvature, the preliminary history being given by Dr. Newton. Discussed by Drs. Sayre, Berg, Newton, Myers, and Gibney.

H. W. Berg, M. D. Paper: "Etiology and clinical phenomena of postpharyngeal abscess." Published: Med. Rec., 1894, xlv., 522. Discussed by Drs. O. B. Douglass, Swinburne, Whitman, Townsend, and Berg.

W. R. Townsend, M. D. Paper: "Treatment of postpharyngeal abscess."

W. B. Coley, M. D. Paper: "The mechanical treatment of hernia." Published: Amer. M. S. Bull., 1894, vii., 460. Discussed by Drs. DeGarmo, Gibney, Walker, and Coley.

APRIL 20TH, 1894.—A. B. Judson, M. D., presented a case of tumor albus wearing a new foot piece on the ischiatic crutch. Discussed by Dr. Gibney.

A. M. Phelps, M. D., presented a case of wrist-drop wearing an appliance to keep the hand in extension. Discussed by Drs. Gibney, Townsend, Shaffer, Sayre, aud Taylor.

S. Ketch, M. D., presented a child that had been operated upon for mastoid disease, followed by paralysis of the opposite side and epilepsy. Discussed by Dr. Whitman.

A. M. Phelps, M. D., presented several cases of hip-joint disease illustrating different points in the treatment.

E. H. Bradford, M. D., of Boston, and R. Lovett, M. D., of Boston. Paper: "Distraction in hip disease." Published: N. Y. Med. Jour., 1894, lx., 129. Discussed by Drs. Shaffer, Gibney, Taylor, Phelps, Ketch, Berg, Townsend, and Sayre.

MAY 18TH, 1894.—A. M, Phelps, M. D., presented a case of hip-joint disease with a cure of the left hip but not of the right; also a case of excision of the acetabulum with perforation. Discussed by Drs. Townsend, Taylor, Shaffer, Bryant, Gibney, and Phelps.

S. Ketch, M. D., presented a case of muscular debility of the muscles of the neck. Discussed by Dr. Whitman.

W. R. Townsend presented two cases of chronic osteitis of the shoulder-joint.

C. Beck, M. D., presented a case of transverse frac-

ture of the olecranon. Discussed by Drs. Bryant, Whitman, and Phelps.

R. Whitman, M. D., presented a case of Charcot's disease following a fracture in a patient with locomotor ataxia. Discussed by Dr. Shaffer.

T. H. Myers, M. D., presented a case of compound fracture of the femur with marked deformity. Discussed by Drs. Townsend, Gibney, and Manley.

J. D. Bryant, M. D. Paper: "The fallibility of human endeavor in antiseptic methods." Published: Amer. Med. Bull., 1894, vii., 709. Discussed by Drs. Gibney, Manley, Beck, Whitman, and Bryant.

J. W. Seaver, M. D., of New Haven, Conn. Paper: "Physical development at Yale." Discussed by Drs. Shaffer, Bryant, Taylor, Beck, and Seaver.

A. M. Phelps, M. D., presented a specimen consisting of a sequestrum of the eighth dorsal vertebra and two inches of the eighth rib removed from a case of laminectomy.

OCTOBER 19TH, 1894.—I. S. Haynes, M. D., presented a case of congenital deformity of the chest.

F. Churchill, M. D., presented four cases of disease about the hip which had been cured. Discussed by Drs. Shaffer, Whitman, Judson, and Gibney.

N. M. Shaffer, M. D., a case of talipes calcaneus following tenotomy of the tendo-Achilles for talipes equinus. Discussed by Drs. Judson, Whitman, Myers, and Shaffer.

S. Lloyd, M. D., presented a case of voluntary dislocation of the knee-joint. Discussed by Drs. Whitman, Shaffer, Judson, Haynes, Townsend, and Lloyd.

V. P. Gibney, M. D. Paper: "The ischiatic crutch in convalescing cases of hip disease." Published: Amer. Med. Surg. Bull., 1894, vii., 1353. Dis-

cussed by Drs. Judson, Myers, Townsend, Ketch, Shaffer, Whitman, and Gibney.

I. S. Haynes, M. D. Paper: "Congenital deformities of the chest." Published: Amer. Med. Surg. Bull., 1894, vii., 1356. Discussed by Drs. Shaffer, Whitman, Ketch, and Haynes.

NOVEMBER 16TH, 1894.—T. H. Myers, M. D., presented a case of rotary lateral curvature in the lumbar region. Discussed by Drs. Whitman, Holgate, Hubbard, Sayre, and Elliott.

L. W. Hubbard, M. D. Paper: "Some unusual nervous symptoms in Pott's disease." Published: Med. News, 1895, lxvi., 206. Discussed by Drs. Judson, Le Fevre, Elliott, and Hubbard.

DECEMBER 21ST, 1894.—S. Ketch, M. D., presented a patient for diagnosis. Discussed by Drs. Judson, Gibney, Taylor, Schapps, Ketch, and Myers.

R. Whitman, M. D., presented a case of talipes equinus upon which he had operated by division of the plantar tissues and by wrenching. Discussed by Drs. Ketch, Phelps, and Myers.

Also a case with congenital torticollis with double congenital dislocation of both hips. Discussed by Drs. Gibney, Phelps, Taylor, and Whitman.

T. H. Myers presented a case of osteomyelitis of the femur. Discussed by Drs. Whitman, Phelps, Gibney, and Lloyd.

A. B. Judson, M. D. Paper: "A method of preventing shortening after hip disease." Published: Amer. Med. Surg. Bull., 1895, viii., 4. Discussed by Drs. Taylor, Whitman, Myers, and Judson.

SAMUEL LLOYD, M. D.,
Secretary.

SECTION ON OPHTHALMOLOGY AND OTOLOGY.

JANUARY 15TH, 1894.—*Cases presented:*

C. B. Carter, M. D. Symmetrical changes in the region of the yellow spot in each eye of an infant.

P. Fridenberg, M. D. Nasal hemianopsia.

J. B. Emerson, M. D. Non-specific interstitial keratitis.

W. A. Vulpius, M. D. Two cases of Stacke's on middle ear.

T. S. Sutphen, M. D. Piece of steel embedded in retina visible with the ophthalmoscope.

E. Gruening, M. D. Paper: "The management of the iris in the simple extraction of cataract." Discussed by Dr. H. Knapp.

FEBRUARY 9TH, 1894.—*Cases presented:*

J. E. Weeks, M. D. Unusual case of extraction of lens.

W. B. Marple, M. D. Atrophy of optic nerve, following blow over orbital ridge.

F. Valk, M. D. Hæmorrhage at macula ("retinitis proliferans").

E. Gruening, M. D. Case of mastoid operation.

Discussion of Dr. Gruening's paper read at last meeting, participated in by Drs. Pooley and C. S. Bull.

A. Duane, M. D. Paper: "Paralysis of the superior rectus." Discussed by Drs. Knapp and Gruening.

MARCH 19TH, 1894.—Clinical evening. *Cases presented:*

E. Fridenberg, M. D. Orbital osteoma.

W. B. Marple, M. D. Peculiar case of parenchymatous keratitis.

J. L. Barnes, M. D. Paresis of internal rectus.

Also: Peculiar retinal detachment.

Also: Injury of the eye.

J. A. Andrews, M. D. Cycloplegia.

Also: Bony choroid.

H. Knapp, M. D. Sebaceous cyst of orbit.

Also: Eye enucleated, showing piece of steel in sclera.

J. E. Weeks, M. D. Retinal detachment, after cataract operation where reattachment occurred.

Instruments presented:

C. H. May, M. D. Combination chart. Also: Cilia forceps.

J. L. Barnes, M. D. Adaptation of maddax rod to phorometer.

W. E. Lambert, M. D. A refractometer.

APRIL 16TH, 1894.—

J. E. Weeks, M. D., presented a case of right hemianopsia with amnesic aphasia and agraphia.

O. D. Pomeroy, M. D. Paper: "Diagnostic and pathogenic factors in labyrinthine disease." Published: N. Y. Polyclinic, 1894, iii., 129.

W. F. Mittendorf, M. D. "Some rheumatic affections of the eye and their treatment."

MAY 21ST, 1894.—W. A. Holden, M. D., presented drawings of the membrana tympani in health and disease.

Cases presented:

J. E. Weeks, M. D. Xerosis of cornea.

W. B. Marple, M. D. Subconjunctival cyst.

O. D. Pomeroy, M. D. Case of aspergillus flavescens.

W. M. Leszynsky, M. D. Demonstration of technique of metallic electrolysis in the treatment of trachoma.

W. B. Marple, M. D. A refractometer.

S. M. Payne, M. D. Paper: "Regular astigmatism." Published: Amer. Med. Surg. Bull., 1894, vii., 711.

OCTOBER 15TH, 1894.—H. Knapp, M. D. Paper: "Recent observations in the eye and ear clinics of Europe."

NOVEMBER 19TH, 1894.—*Cases presented:*

D. Webster, M. D. Cholesterine crystals in the retina.

F. G. Esson, M. D. "Bell's palsy."

J. A. Andrews, M. D. Abscess of ethmoid cells.

W. A. Holden, M. D. Paper: "The probable hæmorrhagic origin of the striated affections of the retina." Published: Arch. Ophthalmol., 1895, xxiv., 147.

F. W. Ring, M. D. Paper: "The combined versus the simple extraction of cataract." Published: Med. Rec., 1895, xlvii., 234. Discussed by Drs. C. S. Bull, E. Gruening, T. R. Pooley, D. Webster, and J. E. Weeks.

DECEMBER 17TH, 1894.—*Cases presented:*

J. E. Weeks, M. D. Disease of the ethmoid cells.

E. Fridenberg, M. D. Peculiarly arranged corneal opacitics following parenchymatous keratitis.

F. Van Fleet, M. D. Paper: "The development of a new point of fixation in the eye."

C. S. Bull, M. D. Paper: "The subconjunctival injection of sublimate solutions in chronic diseases of the eyeball." Published: N. Y. Med. Jour., 1895, lxi., 78.

E. Fridenberg, M. D. Paper: "Dermatitis periocularis medicamentosa (pseudo-erysipelas)." Published: Ophthalmic Record, 1895, iv., 389.

W. B. MARPLE, M. D.,
Secretary.

SECTION ON LARYNGOLOGY AND RHINOLOGY.

JANUARY 24TH, 1894.—J. Wright, M. D., exhibited some instruments for cleansing the nose as used by Prof. W. Meyer of Copenhagen.

J. E. H. Nichols, M. D., exhibited a nasal draw shave.

J. M. Bleyer, M. D., exhibited some electrodes.

J. Wright, M. D., presented a patient upon whom he operated five years ago on account of a hyperplasia of the nose and lip.

R. C. Myles, M. D., showed a case of ranula.

J. E. Newcomb, M. D., showed a case of ranula.

J. E. Weeks, M. D. Paper: "A case of tracheotomy in syphilitic stenosis of the larynx." Published: Jour. Laryngol., 1894, viii., 293. Discussed by Drs. Delavan, Knight, Weeks, Simpson, Bleyer, Park, Myles, Nichols.

J. E. Newcomb, M. D., showed a case of fistula of the neck following abscess.

J. E. H. Nichols, M. D., showed a case of abscess of the larynx, and related a case of a hair in the larynx.

J. Wright, M. D., showed a case of chorditis tuberosa. Published: Jour. Laryngol., 1894, viii., 293. Discussed by Drs. Delavan, Simpson, Gleitsmann, and Wright.

J. W. Gleitsmann, M. D., exhibited a calculus removed from a tonsil. Published: Jour. Laryngol., 1894, viii., 293.

FEBRUARY 28TH, 1894.—D. L. Hubbard, M. D., reported a case of laryngectomy.

D. B. Delavan, M. D., showed a case of laryngectomy.

J. Wright, M. D., showed the remains of a cyst removed from the postnasal space.

C. Beck, M. D., presented a patient upon whom he had resected the hyoid bone for congenital fistula of the neck.

M. E. Hennessy, M. D., presented a patient with an opening about half an inch long in the velum.

H. Knapp, M. D., reported a case of removal of sequestra and a tooth from the floor of the nose. He also read a paper: "On sero-mucous cysts beneath the wing of the nose"; with the report of a case. It was discussed by Dr. W. Freudenthal.

W. K. Simpson, M. D. Paper: "Some considerations on the galvano-cautery puncture of hypertrophied tonsils." Published: Amer. Med. Surg. Bull., 1894, vii., 371. Discussed by Drs. R. C. Myles, J. E. Nichols, E. Mayer, R. P. Lincoln, F. J. Quinlan, H. H. Curtis, and Simpson.

MARCH 28TH, 1894.—O. B. Douglas, M. D., presented a new antiseptic application.

R. C. Myles, M. D., exhibited a specimen of sarcoma of the antrum, and two patients in whom he had opened the frontal sinus. The cases were discussed by Drs. Delavan and Noyes.

W. K. Simpson, M. D., presented a patient with necrosis of the superior maxilla involving the antrum.

D. B. Delavan, M. D., presented a case of successful removal of a nasopharyngeal sarcoma.

R. P. Lincoln, M. D., presented a case in which he had removed a nasopharyngeal tumor by the galvano-cautery écraseur, and reported the result of another case of the same kind. Published: N. Y. Med. Jour., 1894, lix., 653. The case was discussed by Dr. H. D. Noyes.

W. H. Park, M. D., presented a case of adhesion of the posterior pillars of the fauces to the back of the pharynx.

A. Rupp, M. D. Paper: "Fish bones in the throat and what they suggest concerning deglutition." Published: N. Y. Med. Jour., 1894, lix., 609. Discussed by Drs. Delavan, R. C. Myles, W. C. Phillips, and W. H. Park.

APRIL 25TH, 1894.—W. C. Phillips, M. D., gave the formula for an astringent tablet. He also related the history of a case of perforation of the ear-drum following the use of an atomizer in the nose. The case was discussed by Dr. F. J. Quinlan.

W. F. Chappell, M. D., related the history of a case of tracheotomy for acute laryngitis in a case of papillomata of the larynx. Discussed by Drs. R. C. Myles, J. W. Gleitsmann, J. Wright, and Chappell.

W. F. Chappell, M. D., presented a case with syphilitic lesions of the larynx and pharynx.

W. K. Simpson, M. D., presented a case of early syphilitic lesions of the pharynx.

R. C. Myles, M. D., presented a case of deflected nasal septum with adhesions. It was discussed by Dr. T. P. Berens.

J. Wright, M. D. Paper: "A consideration of the vascular mechanism of the nasal mucous membrane and its relation to certain pathological processes." Published: Amer. Jour. Med. Soc., 1895, cix., 516. Discussed by Drs. W. H. Park, J. E. Nichols, E. L. Meierhof, W. K. Simpson, R. C. Myles, W. C. Phillips, and Wright.

MAY 23D, 1894.—Drs. D. W. Hubbard and H. Knapp exhibited instruments.

W. F. Chappell, M. D., related a case of abscess of the thyroid perforating into the trachea.

W. H. Park, M. D., showed a section of a skull with marked deviation of the posterior nasal septum.

J. E. H. Nichols, M. D., presented a patient with

osteosarcoma of the nose. It was discussed by Dr. W. H. Park.

W. C. Phillips, M. D., presented a case on whom he had performed thyrotomy for multiple papilloma of the larynx.

R. C. Myles, M. D., presented a case of myxo-sarcoma, and also one of osteosarcoma.

J. Wright, M. D. Paper: "Some remarks based upon personal observation and experience in the treatment of diseases of the accessory nasal sinuses." Published: Brooklyn Med. Jour., 1895, ix., 354. Discussed by Drs. R. C. Myles, W. K. Simpson, and Wright.

OCTOBER 24TH, 1894.—W. B. Johnson, M. D., related the history of a case of sarcoma of the palate successfully treated by the toxins of erysipelas. Published: Med. Rec., 1894, xlvi., 616. It was discussed by Drs. W. B. Coley and D. B. Delavan.

W. F. Chappell, M. D., presented a case of sarcoma of the epiglottis.

S. Sherwell, M. D., related the history of a case of unusually large tumor (cancer?) removed by the cold snare from the hyoid fossa and base of the tongue. It was discussed by Dr. D. B. Delavan.

R. C. Myles, M. D., presented a patient with malignant growth of the larynx.

T. P. Berens, M. D., presented a patient with a hematoma of the posterior pillar of the pharynx. It was discussed by Dr. J. W. Gleitsmann.

J. W. Gleitsmann, M. D., exhibited instruments of Krause and Herring for the treatment of laryngeal tuberculosis, presented a patient treated by them, and showed specimens of tissue removed. The case was discussed by Drs. Vulpius, Bosworth, Delavan, and Gleitsmann.

NOVEMBER 28TH, 1894.—H. H. Curtis, M. D., exhibited

the Woodruff apparatus for pulmonary gymnastics.

W. K. Simpson, M. D., exhibited a new tonsillotome made by Ermold.

T. P. Berens, M. D., exhibited a new septum knife.

D. B. Delavan, M. D., exhibited a new adenoid knife.

J. W. Gleitsmann, M. D., presented a case of laryngeal tuberculosis cured by Krause's method.

T. J. Harris, M. D., presented a case of fracture of the larynx. Published: Med. News, 1895, lxvi., 208.

R. C. Myles, M. D., presented a case of papilloma of the larynx.

J. E. Nichols, M. D., presented a case of marked deviation of the nasal septum.

H. H. Curtis, M. D., presented a case of disease of the sphenoidal sinus.

A. Rupp, M. D. Paper: "Perforations of the septum." Published: Med. Rec., 1894, xlvi., 771.

D. B. Delavan, M. D. Paper: "Extensive perforations of the bony septum." Published: Annal. Ophthal. and Otol., 1895, iv., 214.

M. J. Asch, M. D. Paper: "The treatment of deviated septum," and presented a case.

These papers were discussed by Drs. T. R. French and Asch.

W. Freudenthal, M. D. Paper: "Rheumatic and allied affection of the pharynx, larynx, and nose." Published: Med. Rec., 1895, xlvii., 196.

DECEMBER 26TH, 1895.—C. H. Knight, M. D., presented a case of fibroma of the nasal fossa.

C. G. Coakley, M. D., presented a case of nasal osteoma.

J. W. Gleitsmann, M. D., reported a case of spontaneous extrusion of the arytenoid cartilage in a case of tubercular laryngitis. It was discussed by Dr. Delavan.

J. Wright, M. D., showed the head of an ox with an enormous tumor in the antrum.

J. W. Gleitsmann, M. D., showed a new conchotome.

F. E. Hopkins, M. D. Paper: "The recurrence of lymphoid hypertrophy in the nasopharynx." Published: N. Y. Med. Jour., 1895, lxi., 109. It was discussed by Drs. J. Wright, W. K. Simpson, R. C. Myles, C. G. Coakley, J. W. Gleitsmann, W. F. Chappell, F. J. Quinlan, J. E. Newcomb, J. F. McKernon, and D. B. Delavan.

J. E. NEWCOMB, M. D.,
Secretary.

SECTION ON OBSTETRICS AND GYNÆCOLOGY.

JANUARY 26TH, 1894.—S. Marx, M. D., presented and described a new incubator.

H. N. Vineberg, M. D., read a report of a case illustrating premature atrophy of the genital organs.

FEBRUARY 22D, 1894.—P. F. Mundé, M. D., presented five specimens of tubal gestation.

H. C. Coe, M. D. Paper: "Notes on the dermatoneuroses of pregnancy, and the puerperium." Published: N. Y. Polyclinic, 1894, iii., 99. Discussion: Drs. Marx, Collyer, McLean, and Goelet.

MARCH 29TH, 1894.—P. F. Mundé, M. D. Paper: "Ovarian abscess." Published: Gaillard's Med. Jour., 1894, lviii., 409.

S. Marx, M. D. Paper: "Further contributions to the study of occipito-posterior positions." Published: Med. Rec., 1894, xlv., 585. Discussion: Drs. Grandin, Tucker, Wells, and McLean.

APRIL 26TH, 1894.—B. H. Wells, M. D. Paper: "Treatment of abortions." Published: Med. Rec., 1894, xlvi., 353. Discussion: Drs. Mundé, Waldo, Collyer, Dudley, Murray, and Currier.

MAY 24TH, 1894.—J. E. Janvrin, M. D., presented a specimen of sarcoma of the uterus, removed by vaginal hysterectomy.

H. N. Vineberg, M. D. Paper: "Congenital annular stenosis of the vagina—an improved method of operating." Published: Med. Rec., 1894, xlvi., 617. Discussion: Drs. Janvrin, Collyer, Ayers, Porter, and McLean.

H. J. Boldt, M. D. Paper: "Abscess of the ovary." Discussion: Drs. Janvrin, Locke, and Currier.

OCTOBER 25TH, 1894.—J. R. Goffe, M. D., presented a specimen illustrating a large ovarian cyst.

H. J. Boldt, M. D. Paper: "Extirpation of the uterus and adnexa in pelvic suppuration, and post-puerperal septicæmia." Published: Amer. Jour. Obstet., 1895, xxxi., 1. Discussion by Drs. Van Ramdohr, Grandin, Goffe, Porter, and Wylie.

NOVEMBER 22D, 1894.—T. J. McGillicuddy, M. D., presented and described an axis-traction forceps.

T. J. McGillicuddy, M. D. Paper: "Entanglements and shortening of the umbilical cord." Published: N. Y. Med. Jour., 1895, lxi., 452. Discussion: Drs. Dudley, Collyer, Marx, Tucker, and McLean.

C. B. White, M. D. Paper: "Surgical treatment of anteflexion of the uterus." Discussion: Drs. Porter, Currier, Jarman, and Dudley.

DECEMBER 27TH, 1894.—H. C. Coe, M. D., presented a specimen of a uterus removed per vaginam by the method of enucleation.

H. N. Vineberg, M. D., presented a specimen from a case of ectopic gestation.

E. A. Tucker, M. D., presented an intra-uterine double-current douche tube.

P. Harris, M. D. Paper: "Studies in pelves, pelvimeters, and pelvimestry." Published: Annal.

Gynæcol. and Pediat., 1895, viii., 243. Discussion by Drs. Marx, Collyer, and Tucker.

GEORGE W. JARMAN, M. D.,
Secretary.

SECTION OF PEDIATRICS.

JANUARY 11TH, 1894.—

Diseases of the Skin in Early Life:

L. D. Bulkley, M. D. Paper: "Common forms of skin affection in early life, with their diagnoses."

J. P. McGowan, M. D. Paper: "Reflex skin diseases in children."

G. H. Fox, M. D. Paper: "General methods of treatment of skin diseases in children."

H. Koplik, M. D. Paper: "Anal fissure and painful erosions of this region in infants and children."

FEBRUARY 15TH, 1894.—

In charge of stated meeting of the Academy.

MARCH 8TH, 1894.—W. L. Stowell, M. D., presented a case of anæmia with visceral enlargements, supposed to be sarcoma.

Sara Welt, M. D., presented a case of persistent omphalo-mesenteric duct.

Bronchitis in Infants and Children:

A. Reich, M. D. Paper: "Complications and differential diagnosis." Published: N. Y. Med. Jour., 1894, lx., 19.

C. G. Kerley, M. D. Paper: "Treatment of bronchitis in children under one year of age."

H. Koplik, M. D. Paper: "Treatment of bronchitis in children."

APRIL 12TH, 1894.—C. E. Nammack, M. D., presented a case of Friedreich's ataxia, the patient being a girl of fourteen years. Published: Med. Rec., 1894, xlvi., 171.

Cerebro-Spinal Meningitis:

H. M. Biggs, M. D. Paper: "Etiology and morbid anatomy of cerebro-spinal meningitis." Published: The Wilder Quarter Century Book, 1893, page 315.

H. W. Berg, M. D. Paper: "Clinical manifestations of the recent epidemic of cerebro-spinal meningitis."

J. Lewis Smith, M. D. Paper: "Diagnosis and treatment of cerebro-spinal meningitis."

MAY 10TH, 1894.—C. E. Nammack, M. D., presented a case of scrofuloderma verrucosa. Published: N. Y. Med. Jour., 1894, lx., 116.

R. G. Freeman, M. D. Paper: "Pasteurized milk as supplied by the Straus milk depot." Published: Med. Rec., 1894, xlvi., 133.

Management of the Breast-fed Infant:

E. A. Tucker, M. D. Paper: "Management of the nipples and breasts before and during the puerperal period." Published: Med. Rec., 1894, xlv., 649.

J. Walker, M. D., of Brooklyn. Paper: "Diet of the nursing mother." Published: Archiv. Pediat., 1894, xi., 416.

L. E. Holt, M. D. Paper: "Signs of inadequate nursing." Published: Arch. Pediat., 1894, xi., 461.

W. L. Stowell, M. D. Paper: "Feeding after weaning." Published: Med. Rec., 1894, xlvi., 65.

NOVEMBER 8TH, 1894.—Sara Welt, M. D., presented a case showing result of operation by ligature, of persistent omphalo-enteric duct.

A. C. White, M. D. Paper: "Antitoxin treatment of diphtheria, based upon a series of cases treated at the Willard Parker Hospital." Published: Med. Rec., 1894, xlvi., 609.

DECEMBER 13TH, 1894.—M. P. Jacobi, M. D., presented a case of congenital ptosis and gave a brief review of the literature of the subject. Published: Med. Rec., 1895, xlvii., 227.

Typhoid Fever in Infants and Children:

W. P. Northrup, M. D. Paper: "Pathology and occurrence." Published: Arch. Pediat., 1895.

W. L. Stowell, M. D. Paper: "Enteric fever in children and infants.—A clinical study." Published: Med. Rec., 1895, xlvii., 321.

H. D. Chapin, M. D. Paper: "Treatment and management of typhoid fever in infants and children."

W. L. STOWELL, M. D.,
Secretary.

SECTION ON NEUROLOGY.

FEBRUARY 16TH, 1894.—C. H. Knight, M. D. Paper: "Reflex neuroses of the rhino-pharynx, with report of a case of torticollis following removal of adenoids." Published: Annal. Ophthal. and Otol., 1894, iii., 161. Discussion: Drs. Butts, Delavan, and Teriberry.

S. J. Meltzer, M. D. Paper: "Recent experimental contributions to the physiology of deglutition." Published: N. Y. Med. Jour., 1894, lix., 389. Discussion: Drs. Einhorn, Thomson, and M. P. Jacobi.

L. Witmer, M. D., of Philadelphia. Paper: "Pain from the psychological point of view." Published: Jour. Nerv. and Ment. Dis., 1894, xix., 209. Discussion: Mr. H. R. Marshall, Drs. C. L. Dana and J. Collins.

APRIL 13TH, 1894.—C. E. Nammack, M. D., presented a specimen of sarcoma of the left frontal region of brain.

Mr. Julius Wolff of Frankfurt, A. M. Paper: "Demonstration of the Wolff method of treating writer's cramp." Discussion: Dr. L. C. Gray.

M. P. Jacobi, M. D. Paper: "Haig's theory of uric acid." Discussion by Mr. F. Smith.

I. Van Gieson, M. D. Paper: "Observations on hæmato-myelo-porus. Read by title.

G. M. Hammond, M. D. Paper: "The significance of the ankle-clonus and exaggerated knee-jerk and their relation to diagnosis." Discussion: Drs. Leszynsky, Jacobi, and Peterson.

OCTOBER 5TH, 1894.—H. D. Chapin, M. D. Paper: "Infantile measurements with special reference to the head." Published: Med. Rec., 1894, xlvi., 649. Discussion: Drs. Booth, Collins, and Peterson.

F. Cabot, M. D. Paper: "Somnambulistic masturbation with report of 3 cases." Published: Amer. Med. Surg. Bull., 1894, vii., 1299. Discussion: Drs. Hammond and Peterson.

JOSEPH COLLINS, M. D.,
Secretary.

SECTION ON PUBLIC HEALTH AND HYGIENE.

MARCH 7TH, 1894.—A. M. Powell, Esq. Paper: "The question of state regulation of prostitution." Discussion: Drs. Roosa, Emily Blackwell, A. F. Currier, A. H. Smith, and Adams.

DECEMBER 5TH, 1894.—Drs. T. H. Manley and D. H. Stewart. Paper: "The efficient and economical disposal of the household garbage of New York." Published: The Sanitarian, 1895, xxxiv., 3. Discussion: Drs. S. B. W. McLeod, Kinch, and S. T. Armstrong.

F. H. DILLINGHAM, M. D.,
Secretary.

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